

**The Research Function of
University Bureaus and Institutes
for Government-Related Research**

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ERRATA

p. 6, last line—*Reads:* Tennessee and Virginia.

Should read: Tennessee, Virginia, and Washington.

p. 7, l. 27— *Reads:* Joseph Harris.

Should read: Joseph P. Harris.

p. 8, l. 1— *Reads:* its off-campus relations.

Should read: relations, and the problems confronting a newly established bureau.

The Research Function of
University Bureaus and Institutes
for Government-Related Research

The Research Function of University Bureaus and Institutes for Government-Related Research

DWIGHT WALDO, *Editor*

Report of the Conference

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Foreword

THIS VOLUME is the product of the Conference on the Research Function of the University Bureaus and Institutes for Government-Related Research, held at Berkeley, August 17–28, 1959.

The Conference was suggested by Gilbert Y. Steiner of the University of Illinois in correspondence with York Willbern of Indiana University and Dwight Waldo of the University of California. These three constituted themselves a Sponsoring Committee to plan and administer and applied to The Ford Foundation for financial support for the venture. The Ford Foundation responded not only with the necessary financial assistance but also, through the good offices of William Pincus, with personal encouragement and assistance. This assistance, financial and personal, is gratefully acknowledged.

It is important to note that the Conference was centered in the *research* function of the bureaus. No attempt was made to deal with the teaching, clearing house, consulting, and other functions that the bureaus perform, except as these other functions relate, positively or negatively, to the research function.

In addition to the twelve participants proper, who were in attendance throughout the conference, various “consultants,” not members of bureaus or institutes, were present for periods of one to three days, for discussions in the areas of their expertise. To expand the area of information, criticism, and advice still further, each paper prepared for the Conference was submitted to a second person for “comments” in writing. (Writers of comments, except for one “participant,” were not present at the Conference.)

The topics for papers—and hence for discussion at the Conference—were selected by the Sponsoring Committee in accordance, not with necessity or logical consistency, but with their “sense of the situation.” Accordingly, some of the papers deal with a “field” as conventionally labeled, while others deal with currently prominent or emerging research interests of the bureaus, and one is addressed to research methods.

They are presented in this order below, each followed by the Comment thereon.

The "Summary Statement by Conferees" was framed at the conclusion of the Conference. It reflects, inevitably, the still unresolved clash of differing points of view. But he who reads it carefully will gain important information about where the "bureau movement" now stands, and the directions it is likely to take, in research, in the years ahead.

The introductory paper by Amin Alimard was prepared especially for this volume. Mr. Alimard was not present at the Conference. However, since he was engaged in writing a doctoral dissertation dealing with the history and status of the bureau movement, he was invited to contribute this essay to provide a "setting" for the papers. Incidentally, those wishing more information about the bureau as an institution and its relations to its institutional and intellectual environment, will find the forepart of the essays by Harvey C. Mansfield and Gilbert Y. Steiner highly rewarding.

I record my warm thanks to my Administrative Assistant, Mary L. Sisson, who gave invaluable aid both in the organization of the Conference and in seeing these proceedings into print.

DWIGHT WALDO

Berkeley, California
August, 1960

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AMIN ALIMARD¹

Origins, History, and Directions of the University Bureau Movement in the United States

A popular government without popular information,
or a means of acquiring it, is but a prologue to a farce
or a tragedy.

James Madison

THE "UNIVERSITY BUREAU" is a product of twentieth century urbanism in the United States. It is one of the many threads in a complex pattern of responses by which governmental reform, particularly at the local level, has been accomplished in the past sixty years. Philosophically, it is perhaps fair to say that the university bureau is an expression of the belief in *facts* as a basis for rational behavior. The earliest reform attempts tended to center around individuals, who either failed utterly in their mission or whose reforms tended to be short lived because institutions and social systems cannot be easily changed by mere force of personality. Thus research—a striving for objective data—came to occupy an important position in the reform framework, and it is not surprising that the universities should have become important participants in this effort.

It is not surprising, either, that such a general charter would stimulate many different kinds of approaches. The fact is that there is no "pure" model of the university bureau. There are differences on every dimension: concept of function, organizational arrangements, size, resources, personnel, and so forth. What holds this heterogeneous group together is the general commitment to secure and convey information to decision-makers for public policy and management purposes. Unless an organization feels this basic responsibility, it seems hardly appropriate to think of it as a "university bureau."

¹ The author is a faculty associate in the Institute for Administrative Affairs, University of Tehran, Iran. This essay is a summary of some of the material gathered for a doctoral dissertation on university bureaus. The dissertation, for presentation to the School of Public Administration, University of Southern California, is now being written.

More specifically, it should be noted that we are concerned in this paper only with those institutions which have a *direct* connection with the universities. It must be noted, however, that many other organizations, ranging from the famed Brookings Institution in Washington, D.C., to the many taxpayers' associations, are engaged in pursuits which are somewhat similar and serve the same social purpose. The term "bureau" also has a special meaning. To some extent it may convey the notion of research, but it also happens to be the name which is most often given to the University units engaged in such fact-finding endeavors. Finally, the name suggests a special organizational arrangement which is separate from the normal teaching and research pursuits of the university. In sum, this paper is concerned with the development in universities of formalized organizations which have as one of their principal objectives the securing and dissemination of information pertinent to public questions, both of a policy and a managerial nature.

THE GENERAL BUREAU MOVEMENT

As has been indicated, the Bureau Movement in the universities was part of a much broader environment of government reform, marked most dramatically by the establishment in 1906 of the New York Bureau of Municipal Research. Though not officially connected with a university, the New York Bureau had a close link with the academic world. One of its early participants was Charles A. Beard, for many years a professor at Columbia University. According to the formula of the day, the Bureau was ideologically committed to the separation of politics and administration. Politics might be irrational and emotional; but administration need not be. Administration was potentially a *science*, rooted in the efficiency value. It is quite apparent that this belief system provided great motivation for research. It suggested the presence of truths, patterns of conduct, and almost automatic decision-making in a broad arena of government. Tenable or not, it was a vision that many people at that time undoubtedly needed in order to inspire them to move local governments out of the depths of corruption.

The New York Bureau gave much of its early attention to budgeting, where reform was badly needed in almost all governmental jurisdictions. As a result of the Bureau's recommendations, the City of New York in 1907 adopted a segregated, functionalized budgeting system. In those early "honeymoon" years, the Bureau also made recommendations on purchasing, procedures, and organization. Quite a number of these proposals were adopted, and these successes were enough to arouse the hopes of reformers across the country. Less work came to be done in New York and more in other jurisdictions. Perhaps more importantly,

the New York Bureau became a model for similar agencies. Philadelphia, Chicago, Cincinnati, and Milwaukee—to name only a few—followed the eastern example.

Most of the early bureaus were privately supported. A segment of them consisted of independent, citizen-financed research agencies. Another segment was composed of research units in the chambers of commerce, taxpayers' associations, or other civic organizations. Governments, too, created research units dedicated to the principle of efficiency. By 1914 these proliferating governmental research agencies established a national federation, the Government Research Conference. In 1917 the Bureau of Government of the University of Michigan, serving informally as the secretariat since 1914, became the central office and clearinghouse for the Conference. A year later a monthly bulletin, which reported on the work and research projects of the affiliated agencies, began publication.

The acquisition and maintenance of documents became increasingly important as research interest heightened. Chicago was the first formally to establish a municipal reference library, in 1900. After that, the growth of such specialized government collections paralleled the development of the Bureau Movement itself.

From this brief review it is apparent that the Bureau Movement in general involved much that was compatible with the over-all mission of the universities. It implied research, publications, community service and consulting, library facilities, and training. Furthermore there was in the rationale behind the movement an aura of objectivity and neutrality, based primarily on the separation of administration from politics. This obviously was of great importance to the publicly supported universities, necessarily politically sensitive. Finally, it should be noted that the movement was largely grass-roots based. It emphasized reform of local governments. As a result nearly every university and college could find an appropriate research need and locale for its efforts.

THE UNIVERSITY BUREAU MOVEMENT

The beginning of the University Bureau movement may be set as 1909. This was the year the University of Wisconsin established a Municipal Reference Bureau in its extension division. It was an ambitious undertaking, aiming to collect and furnish information on all phases of city government, from administrative problems to public works, control of public utilities, smoke abatement, and the like. The Bureau also gathered and maintained a file of charters and ordinances of major cities in the United States, as well as European materials. It

had correspondents in most of the principal cities in support of its clearinghouse function. In 1912 the Bureau reported it answered over 1,500 inquiries from every city in Wisconsin and from every state.

From the inception of the Bureau there was an organic relationship between the League of Wisconsin Municipalities and the Bureau, the Director of the Bureau serving as secretary of the League. He also was the editor of the official magazine of the League, *The Municipality*. All this was accomplished on an annual budget of about \$5,000.

Wisconsin's claim to the first university bureau can quite properly be contested by Kansas, which established an organization at about the same time. As closely as can be determined by the author, however, the Kansans got under way about two months later in 1909. Actually these two land-grant Universities created Bureaus which were strikingly similar: location in the extension division, clearinghouse functions for municipal information, and with a formal tie-in with the league of municipalities.

Within four years bureaus of a similar character had been established at five other major land-grant Universities: Illinois (1911), Washington (1912), California (1913), Oklahoma (1913), and Minnesota (1913).

In the private universities of the East there was a somewhat different response to these new demands on the universities. In 1911 Harvard organized a Bureau for Research in Municipal Government, financed by an annual gift of \$2,500 from two alumni. In this case, however, the essential purpose of the Bureau was to improve the quality of academic instruction, rather than to provide a community service. In the same year, 1911, Columbia University created a Politics Laboratory which functioned much like the Harvard Bureau. In both Universities there was developed a comprehensive library of public documents, constitutions, laws, statutes, charters, periodicals, and other fugitive materials on municipal affairs.

The almost exclusive interest in municipal affairs began to be diluted in 1911 with the establishment of the Legislative Reference Bureau at the University of Nebraska. Financed on a budget of about \$5,000 per year, the Bureau was expected to serve the needs of the State Legislature as well as those of the municipalities. The Nebraska Bureau was also important because of its formal affiliation with the Department of Political Science and Sociology and with the College of Law. Variations in intra-university organizational relationships also began to emerge. In 1913 two of the five bureaus established by land-grant universities moved away from the extension division arrangement. The Bureau of Municipal Research and Reference at the University of Texas was made completely independent, and the Bureau of Reference and Research in

Government was placed in the Department of Political Science at the University of Michigan.

By 1915 various types of bureaus or reference libraries were operating in the following institutions, in addition to those already mentioned: Indiana University, University of Cincinnati, Western Reserve University, University of Oregon, Grinnell College, and Whitman College.

With World War I the interest in efficiency began to languish. The waging of war superseded all other concerns; and the first period of the University Bureau Movement came to an end. In retrospect, it would seem fair to say that the purpose of these new bureaus was not to serve a scholarly end. Rather their justification was one of community service. It is not surprising, therefore, that the large land-grant universities of the Midwest were the very obvious leaders in the movement. The East, with a relatively small land-grant college system, did not witness the same development. In the South only Texas created a bureau, thus providing evidence of slower urbanization and less attention to reform and efficiency below the Mason-Dixon line. It is important to realize, however, that the early bureaus were very small-scale operations. The size of the budgets reveals that clearly. Thus the presence or absence of one highly motivated person typically was responsible for the creation of the agency in the first place and any vigor it might have possessed in its formative years.

THE BUREAU MOVEMENT BETWEEN THE WARS

The twenty-three-year period between the two world wars was noteworthy in several different respects:

1. The number of bureaus operating in United States universities increased considerably.
2. The horizons of the bureaus tended to move beyond the municipalities toward the higher levels of government.
3. Research interests began to develop somewhat beyond institutional and legal description.
4. The bureaus were less tied to the extension divisions.
5. The bureaus began to play an important role in the public service training effort.

In untold ways World War I left its mark on the society. One obvious imprint was the tremendous growth in importance of the national government. It was inevitable that the government-oriented people in the universities would begin to feel a responsibility for research in this area as well. This change in perspective was observable in the renaming of two bureaus in 1918 and in the naming of a new one in 1919. The Texas bureau became the Bureau of Government

Research and the Michigan unit became the Bureau of Government. Minnesota created the Bureau for Research in Government in the following year. This concern for the higher levels of government was also reflected in the nature of studies undertaken. Social welfare programs, education, and resource conservation are only three examples of the types of studies which some of the bureaus began to make.

After the war, too, public administration began to appear as an identifiable area of study. The first graduate school of Public Administration was established at Syracuse University in 1924. Two years later the first textbook by Leonard D. White of the University of Chicago appeared. Thus one of the original premises of the Bureau Movement, that administration is separable from politics, secured increasing academic support. It was inevitable that such a major intellectual development would be reflected in shifts in the university bureaus. A leader in the movement forward was the Bureau of Public Administration of the University of California (Berkeley), established in 1921 under the dynamic leadership of Samuel C. May. Linked through joint appointments to the Political Science Department but operating independently of it, the Bureau has become known particularly for its outstanding library.

Training activities of the bureaus also gained new emphasis during the 1930's. The Institute of Local and State Government at the University of Pennsylvania and the Institute of Government at the University of North Carolina engaged in large operations, involving formal courses, conferences, and institutes. Stimulated greatly by the Tennessee Valley Authority, the universities of the South now began to develop new programs. Most active at this regional level was the Bureau of Public Administration of the University of Alabama, which helped to create bureaus in the other state universities.

In the two-decade period between the wars, nearly thirty bureaus were established in American universities. In some cases, however, the new units resulted from reorganizations or replacements of previous agencies; and in a few instances more than one agency which deserved the label of "university bureau" was operating on a single campus. Such was the situation at the University of Minnesota and at Indiana University. Bureaus of various types were established at the following institutions: Colorado, Harvard, Massachusetts Institute of Technology, North Carolina, Florida, West Virginia, Alabama, California (at Los Angeles), Connecticut, Hawaii, Indiana, Kansas State, Kentucky, Louisiana State, Maine, Massachusetts State, Mississippi, New Hampshire, New Mexico, Oregon, Pennsylvania, Pennsylvania State, Pittsburgh, Princeton, South Dakota, Temple, Tennessee, and Virginia.

DEVELOPMENTS SINCE WORLD WAR II

Since World War II, the number of university bureaus has grown considerably. But of course the most significant element of this third period was the environment in which the bureaus were functioning. First, there was World War II and the continued significance since that time of military defense and weapons-oriented research. The unprecedented prosperity of the United States over the past fifteen years also tended to make less important the problems of government and relatively less attractive a career in the government service. Intellectually, too, there were profound changes. The behavioral sciences, through such agencies as the Institute of Social Research at the University of Michigan and the Institute of Applied Social Research at Columbia University, provided new and more sophisticated means of gathering and handling data on important public questions. The field of public administration itself underwent important intellectual change. The idea of a dichotomy between politics and administration was widely discarded, at least, in theory. There was a growing awareness of the dynamic and behavioral aspects of public management.

It would be encouraging to report that developments in the university bureaus fully reflected an awareness of these changing times. Unfortunately this does not seem to have been the case. In the five years immediately after the war much was done of a self-appraisal nature. But in the decade of the fifties the mood of criticism and questioning seemed to be replaced by one of "normalcy."

Actually the appraisal movement got under way in 1940 when a committee of the American Political Science Association chaired by Joseph Harris discussed the bureaus and their activities.² Then in 1946, at a joint meeting of the American Political Science Association and the American Society for Public Administration, eight papers on the Bureaus were presented at two round-table sessions. Roscoe C. Martin, then Director of the Bureau at the University of Alabama, organized the program and the papers were published by the Alabama Bureau.³ These papers were divided into several areas. The first three dealt generally with public administration as a special field, public administration and related fields, and the school of public administration as an answer to special needs. Others discussed more specifically the bureaus and focused on the bureau as an answer to special needs, its place in the administrative structure of the University, its work pro-

² The first attempt to look into the University Bureau Movement was made by W. B. Munro's special committee of the National Municipal League in 1915.

³ *The University Bureaus of Public Administration*, University, University of Alabama, Bureau of Public Administration, 1946.

grams, and its off-campus relations. The symposium, however, lacked any general review of the movement. Papers tended to be limited to the author's personal experiences in his organization.

Later in the year, the University of Virginia Bureau, under Rowland Egger, took the leadership in the reappraisal process. In December 1946, Egger convened a group at the next meeting of the American Political Science Association to appraise critically the bureaus in terms of their research product, service, educational activities, and benefit to operating agencies, professional associations, and private consultants.⁴ However, the criticism was not as sharp as Professor Egger had apparently hoped.

The results . . . were less sanguinary than reasonable expectations promised yet were withal constructive and undoubtedly contributed worthily to the expansion of organized knowledge about the institution and its habitat.⁵

The discussions were in the main favorable to the bureaus and their accomplishments. Problems identified included lack of staff to undertake certain responsibilities, failure to utilize adequately other facilities of the university, not enough fundamental research, poor techniques of management analysis, and ineffective performance as a synthesizer of public policy alternatives.

In February 1947, with the assistance of a Rockefeller grant, the University of Virginia convened the directors of fourteen bureaus from across the nation for a four-day session in Chicago, along with representatives of various professional associations. These deliberations revealed a general consensus that the bureaus had been quite effective public relations vehicles for their respective universities and had contributed to the improvement of government in their areas. They had not, however, added anything to the intellectual content of public administration. This session was supposed to have been the beginning of a rather ambitious research project, involving a series of case studies of various bureaus and a summary and evaluative volume. But the directors, still plagued with staffing problems from the war, decided to postpone the project for eighteen months. The reappraisal movement did not regain its original zeal. Rowland Egger and Weldon Cooper did bravely author a 1949 case study of the Alabama Bureau, titled *Research, Education, and Regionalism*, which was the only concrete result of what might have been a very imaginative contribution to the literature of the Bureau Movement.

⁴ *A Critical Appraisal of University Bureaus of Public Administration*, Charlottesville, University of Virginia, Bureau of Public Administration, 1948.

⁵ Rowland Egger and Weldon Cooper, *Research, Education, and Regionalism*, University, University of Alabama, Bureau of Public Administration, 1949, p. vi.

Even though the Bureau Movement seemed to have stalled intellectually, there was no dearth of converts to the old, well-tested pattern. Several new bureaus were established, some were reorganized, and a few became revitalized. Although the line between these processes is rather hard to draw, and the variations among the new bureaus are great, it would appear that essentially new programs were established at the following colleges and universities: Boston, Florida, Florida State, Georgia, Idaho, Illinois, Iowa, Maryland, Massachusetts, Michigan State, Montana State, Nevada, Rutgers, South Carolina, Southern Illinois, Utah, Vermont, and Wichita. A new Bureau at the University of Arizona was expected to begin operation in the summer, 1960.

CONCLUSIONS

Quite recently, the writer made a survey trip across the country to examine the status, programs, and operations of the university bureaus. He visited over thirty bureaus ranging from one-man operations to those with a staff of about forty people. As of 1960, it was apparent that no universal model or pattern for the bureaus had emerged. They were shaped according to the needs and requirements of the area or the state in which they were located. To be sure, all seemed to have moved away from the old municipal orientation and now to provide services for all levels of government. The service orientation remained strong; and perhaps primarily because of this, the Bureau Movement was strongest in state-supported universities. Only a few private universities had bureaus of government research, and these bureaus were either inactive or existed primarily on paper.

Taking the bureaus as a group, there appeared to be little dynamism in the movement. Yet there were about half a dozen revitalized, aggressive, research-oriented bureaus which were assuming a leadership role. Further, two or three large, service-oriented bureaus were making major contributions in the area of in-service training and consulting. But others had remained static, small, and uninspired.

Most of the bureaus seemed apprehensive of any deep involvement in the processes of public policy and community decision-making. The politics-administration dichotomy, although supposedly an outmoded concept and a fallacy in the modern theory of public administration, still lived in the perceptions of most bureau officials. The majority of the bureaus were concerned with administration in the mechanistic, efficiency sense. And thus the old frame of thinking of the early bureaus of fifty years ago has strongly persisted. The justification of the bureau people for this attitude was that they believed, as employees of public-supported institutions, that bureaus should not "interfere" in politics.

One important change was noticeable. There was an unmistakable movement in the direction of more fundamental research. Some bureaus were becoming more interested in comparative studies, community research, and in developing a behavioral approach to governmental problems involving interdisciplinary cooperation.

Research in Public Administration

PART OF WHAT I can say here depends on the fact that it concerns the University bureaus and part on the fact that my subject is research in public administration, while others are to deal with related topics. It seems appropriate to begin, therefore, with a reminder of some special features of the bureaus, since this has a bearing on all the other topics to be considered.

The University bureaus of government-related research number between thirty-one and fifty-three, according to the criterion of vitality used in the count. In size they range from some that amount to not much more than the part-time services of one or two faculty members, with such student assistance as they can muster, to one that has a budget of nearly \$200,000. This is a figure that would support a quite respectable teaching department of political science in most academic institutions, but it is also plain evidence that none of the bureaus has the resources at present to undertake large-scale "operations research," or to play in the same league with the major federally supported or foundation-supported research agencies in the social—let alone the natural—science fields. Taking the publications of the bureaus as a visible yardstick of performance, the output of thirty-one bureaus reporting in a survey covering the past five years ranged from four to seventy-one items, with fifteen a typical figure, of which perhaps three or four were of substantial research proportions.¹ From this it appears that the impact of the bureaus must depend on the quality or wide utility of their publications rather than on their quantity.

With only a handful of exceptions (apparently explainable as the historical accidents of particular personalities or locally available financial backing in a few private institutions), the bureaus are attached to state universities. Not every state, much less every state-supported uni-

¹ I am indebted for the information in this paragraph to two memoranda: one by Stanley Scott of the Bureau of Public Administration at Berkeley, dated November 18, 1958, and the other by Robert H. Pealy of the Institute of Public Administration of the University of Michigan, circulated as Appendix C, "Publications Programs of University Bureaus of Governmental Research," to the Proceedings of the Conference of Directors of University Bureaus of Governmental Research, Washington, D.C., April 1, 1959.

versity, has one; but they are sufficiently rare elsewhere to mark this as their native habitat. No predominant type of state, or geographical concentration, is discernible in the array. This is some evidence, on the one hand, that what the bureaus do, in the way they have been doing it, is regarded as either not worth doing, or as financially infeasible, in most private institutions; and testimony, on the other hand, at the very least, that state legislatures or governing boards can frequently be persuaded to support in a niggardly way over the years what they will neither abolish nor support adequately to the need—the characteristic fate of many worthy institutions of low visibility, dependent on the public purse.

But if this is to say that the potentialities of the bureaus are tied to the futures of the state universities, there are good reasons for optimism in that prospect. In the progress they have made during the past two decades in broadening their outlooks and increasing their outlays, the state universities do not suffer in comparison with other types of academic institutions. And among American governments in recent years, outside the areas into which federal defense funds have been poured, the rising curve of state activities in general, and of state support for higher education in particular, has sloped more sharply upward than for other units in our federal system.² It seems reasonably clear that if the traditional work of the bureaus is to be continued, there is no likelier location for most of it than in the state universities, and that if it should be better done, the state universities have the resources for improvement in prospect. Alternative official locations, in state legislative reference services or in the finance or other staff agencies of state and metropolitan municipal governments, may well have superior technical facilities for some of the sorts of studies that the university bureaus have made, and when they step in, their contributions are welcomed. But on the record they ordinarily operate under at least as severely inhibiting restraints, for the purposes of scholarly research, as the campus-based bureaus. Alternative private locations, except in some functional fields such as public health, have found that financial support is sporadic, or that when it was forthcoming, as in the case of taxpayer organizations, editorial influences were introduced, more constraining than those to which state universities are subject.

Again with few exceptions, the work of the bureaus has been chiefly devoted to state and local governments and to service and training activities in those fields. This, too, is reflected in their publications. In a

² The American Assembly, *The Forty-Eight States: Their Tasks as Policy Makers and Administrators*, New York, Columbia University, 1955, pp. 19–24, drawing on recent census figures and other sources.

sample of their bibliographies an occasional lecture by a distinguished outside scholar brought to the campus for that purpose stands out as a bit of institutional advertising, and there are some substantial monographic studies and works by regular faculty and staff members addressed to broader themes with theoretical significance. But the tone is set by a pragmatic emphasis on products for which a felt clientele need has been expressed and which lie within a ready capability: manuals and reference guides for officials and citizens and citizen groups; descriptive accounts of local institutions; and studies (sometimes with action recommendations) regarding perennial issues of organization, processes, and policies in the domain of state constitutions and lesser public authorities. Similarly, the bureaus have afforded vocational training and found thesis topics and career outlets for graduate students headed toward government service, but they have not found the research reports of their graduate students publishable.

The service orientation of the bureaus ordinarily is candidly avowed, as may be seen from typical responses to a question concerning the objectives of their publications programs. These are variously described as:

... directed ... to the client who requests a specific piece of work. ... [or as designed to] meet the needs of elective and appointive local government officials ... [or] increase the knowledge of professional students of government about the governmental process. ... [but more ambitious aims are sometimes stated: to] contribute to informed consideration of the issues regarding public policy questions. ... [and to] contribute to the development of a systematic theory and to the improvement of research methods in politics and similar areas.³

It is easy to decry this utilitarian emphasis and to disparage the intellectual quality of the bureaus' work. A friendly and perceptive critic has asked, indeed, whether there is not

... some kind of Gresham's law of research ... under which work with a lower theory content drives out that with a higher content, even when both are mixed within the same institution? Are the bureaus fighting a hopeless battle when they try to do basic research and service research at the same time? Do the bureaus serve as receptacles for cheap currency, thus enabling the regular academic departments to stay on the gold standard?⁴

Before indulging the temptation to condescend, we should bear in mind some elements of a broader perspective. In moving into their service functions, the bureaus were entering a vacuum that badly needed filling and that by and large still does. They have never had the funds

³ Robert H. Pealy, *op. cit.*

⁴ Stanley Scott, *op. cit.*

and staff to accomplish more than a small part of the minimum requirements for the orderly assembly, analysis, and dissemination of basic information about the political institutions and people of the states in which they operate. They have not had a tithe of the state (let alone federal) support given to agricultural extension and research or of the resources of state departments of education (apart from grants and earmarked funds) for the study and improvement of local school systems.

The same is true with regard to the advice the bureaus have given about reorganization, reapportionment, administrative techniques and the like. If some of this—particularly from the earlier years of their operations—now looks in retrospect naïve or doctrinaire, it nevertheless reflected fairly faithfully the state of the art and the precepts of the textbooks of the time. We are told now—to pick an extreme analogy—that George Washington went to his death because he took the advice of the two of his three doctors—a majority opinion—who supposed, in accordance with the received doctrine of his time, that bleeding him was the cure for what ailed him. There is competent medical authority, too, for the statement that not until 1910 or 1912 were the chances better than even that a randomly selected patient suffering any randomly selected serious ailment, who put his case in the hands of a randomly selected physician in this country, would emerge better off for the treatment he got. An appreciation of this statistic may have been at the root of the persistent and widespread prejudice of the poorer classes against going to hospitals when they were sick: hospitals were where people died. So, too, the voters have regularly rejected advice from professional reorganizers. But surely the more rational lesson to be drawn, for the bureaus as for medical practitioners, is not to give up the goal of service, but rather to keep as near as possible to the moving frontiers of new knowledge in their field and adapt their practice and advice accordingly. It would be paradoxical in the extreme if those who are closest to the observation and study of political and administrative phenomena at first hand in the field should—over an extended period—be either blind to the reality and significance of what they see or insensitive about what to look for.

It is much less true now than two or three decades ago, and for a variety of reasons, that the horizons of the state universities are defined by the state and local governments that formed their original clienteles. Their faculties have shared in the general increase in mobility. Segments of the vast apparatus of the national government are accessible to them. Some of them are recipients of federal grants, and even of grants from the large private foundations, for research. A few,

through International Cooperation Administration contracts, share the burden of carrying technical assistance in administrative practice to faraway places. At the same time, right at home, the ferment of expanding state activities and metropolitan problems is putting new wine in the old bottles that have long been the bureaus' stock in trade. It seems reasonable to expect, therefore, that the institutional subject matter within the ken of the bureaus' research concerns will continue to expand in geographical terms and to subdivide further functionally, but is unlikely to shift its center of gravity in state and local governmental affairs.

With few exceptions, once more, the bureaus are attached—or closely linked—to departments of political science. An alternative campus location may be found—or conceived of—in a department of sociology or economics or social administration, in a college of law or education, in a school of architecture that emphasizes community planning, or in an interdepartmental organization controlled by some combination of these. To name these alternatives is to suggest why none of them—unless the last—is likely to meet more than a small fraction of the need for research in public administration; their central concerns lie elsewhere, though each has a legitimate potential bearing on it. The interdepartmental alternative is attractive on paper, but it suffers all the handicaps of any league of sovereign powers, such as campus departments ordinarily are. Apart from special infirmities in particular cases, the department of political science is still the logical base for the bureau, and the bureau will show its imaginative understanding of its potential role in the degree to which it exhausts the possibilities of enlisting the collaboration of related disciplines in planning and carrying out its projects.

For some purposes the state universities are organized among themselves across state lines by agreements which, if they have less standing in law than interstate compacts, are nevertheless sufficient for some operating cohesion in matters of common concern such as accreditation, intercollegiate athletics, lobbying for or against federal legislation, and the like. Is there a prospect of interbureau cooperation research through, say, some 1313-type of confederacy that could be brought to bear on topics in comparative state or municipal administration of the magnitude, say, of Paul David's cooperative study, under American Political Science Association and Brookings Institution auspices, of presidential nominating politics in 1952? Barring a philanthropic miracle, but not barring an occasional *ad hoc* bilateral or multilateral arrangement for a particular project, it appears that, as in the case of interdepartmental governing boards on a single campus, the sense of urgency

in a common purpose of sustained comparative research is unlikely to surmount the atomistic tendencies of so many state sovereignties.

Broadly speaking, these appear to be the major institutional restraints on the pattern of university bureau research in public administration. They leave a great deal of room for choice. Can any more positive suggestions be offered to guide that choice?

Three sorts of suggestions come to mind, only to be rejected on reflection as unhelpful in the present context. I shall therefore not dwell on them.

The first of these is exhortation about the need for more research. There is a place for preaching: at ceremonial gatherings within the profession, in appeals for understanding and support directed toward lay brethren outside it, and to incoming students. Preaching may be optimistic or reproachful; it may be reasoned as well as emotional. It may be buttressed by a review of past research, current trends, and remaining inadequacies. But anything I could say along these lines would be repetitive and addressed to the wrong audience. Let me applaud here two well reasoned and complementary essays on the need for research in public administration: one by Frederick C. Mosher⁵ and the other by John C. Honey.⁶ Honey takes issue with Mosher's hope for the cultivation of the field as a better-defined scientific discipline, regarding it as basically an art, but both are agreed on the need for more research.

The second is the further specification of subject matter areas, and of topics within them, as needing research or more research. This may be reduced to a game for idle hours, as in the pages of *Prod*. It may also be tackled systematically and imaginatively, as Morton Grodzins did a decade ago in the field of human relations.⁷ It is particularly helpful when, as in that case, a body of new knowledge has grown up outside the ken of the traditional discipline, the existence and applicability of which has not been appreciated, or when a major shift in intellectual or moral premises makes a general reconsideration of previous estimates of relevance and utility in order, as happened during and after World War II. It is in order, too, whenever an accession of resources for research warrants a fresh look at potentialities and priorities. In any of these situations, suggestions may lead to fruition. I forswear the attempt here, however, again as repetitive in the light of the very recent

⁵ Frederick C. Mosher, "Research in Public Administration: Some Notes and Suggestions," *Public Administration Review* 16: 169-178 (Summer 1956).

⁶ John C. Honey, "Research in Public Administration: A Further Note," *Public Administration Review* 17: 238-243 (Autumn 1957).

⁷ *Human Relations in Public Administration: An Exploratory Report on Research Possibilities*, Chicago, Public Administration Service, 1949, Mimeo., 18 pp.

work of the ASPA Committee on Research Needs and Resources,⁸ and because curbstone observers work in a vacuum when, without any personal responsibility for the outcome, they set about thinking up projects for other people in other circumstances to undertake. There is no dearth of possible subjects, and a research project must first of all capture the intellectual interest of the researcher; no known formula governs the path of curiosity.

The third is some answer to the frequently repeated complaint about the "inadequacy of the communication system" for reporting inventories of research in progress, disseminating abstracts, etc. On this I can only express the opinion that things are likely to get worse before they get better, and that mechanical remedies more ambitious than the sort of bibliographical aids published in the *American Political Science Review*, or the abstracts published in *International Political Science Abstracts*—efforts such as the State Department's *Digest of External Research* in international relations, for instance—will be self-defeating for the want of time or patience to peruse them. I am informed by a colleague in chemistry, a scholar of known probity, that in that field, which is served by a mammoth abstracting service that covers hundreds of journals reporting hundreds of thousands of investigations, it is ordinarily easier to design and carry out an experiment—if a few handy reference checks fail—than to find out definitively whether someone else has already reported performing it. In public administration research the mobility of the profession, the grapevine, and the old system of smoke signals will continue to be prime transmission methods for current intelligence.

Immediately practical suggestions failing, can any considerations of a more theoretical nature be stated to help guide research? It appears to me useful to attempt to distinguish some different types of theory about administrative institutions. Since any kind of generalization is some kind of theory, such distinctions should bear on the selection and organization of facts (i.e., research) and aid in clarifying the sorts of generalizations they may be expected to yield upon study. Put another way, this is an exploration of the limits of science in administrative studies.

⁸ See especially a memorandum by Dwight Waldo to this Committee, dated July 21, 1958, entitled "Neglected Areas of Research," which discusses briefly eight general areas, sixteen more specific suggestions, and ten additional items. See also a memo by Frederick C. Mosher, dated March 16, 1959, on "Research Needs in Public Administration," which stresses the criteria of "priority," "researchability," and "means" in selecting projects for further analysis and design, and a memo by Carl F. Stover, dated August 11, 1958, entitled "Notes on Research in Public Administration," which identifies several recent trends, with examples. All three are comments on a Committee working paper, "Summary of Committee Comments on Issues in Public Administration Research," prepared by Chairman Donald C. Stone and dated March 17, 1958.

The assembly of "raw" facts—for instance the code provisions of a state reciting the powers of its highway department, or a table of the tolls charged by turnpike authorities—is the indispensable and, by itself, intellectually insignificant beginning. There are too many of them for anyone to notice them all and they are relatively cheap because they require only extraction and contain little or no value added by manufacture, as an economist might say. A first step toward adding some of that value and a first step toward generalization is to notice them repeatedly, say in a time series or over a geographical area, and to compile them accordingly. They then become statistics, a compact way of summarizing and classifying them that has the intellectual advantage of making them susceptible of mathematical manipulation, correlation, etc. If facts in this form are collected and published by a university bureau without more, it can only be in the hope of justifying its existence—as does the Census Bureau—by the service of making them available to others who have an interest in them for their own purposes. The criteria of selection thus are largely external—accessibility and supposed clientele interest. If such facts are published as a by-product of further research endeavor, the burden of justification is easier because other criteria support the selection. All this is elementary and preliminary to the point that this is the threshold of the realm of the behavioral studies. If the facts are collected and selected and arranged in the form of inductive generalizations, or are selected and observed so as to test hypotheses empirically, we emerge with some behavioral science, with some scientific theory of the practice of individuals or the working of institutions, including administrative organizations.

This sort of theory is, in principle, exact and verifiable and attractive for that reason. On the assumption of some sort of order in the universe of behavior it invites the construction and testing of statements of wider and wider degrees of generality and the identification of relationships among them. By abstraction from them, models may be drawn and "systems" discerned. To the extent that scientific theory adequately describes the facts it also predicts more like them and in that sense "explains" them.

The range of administrative studies open to scientific investigation in this fashion is potentially as wide as the universe of administrative behavior and in actuality extends far beyond the reach of the research resources that could conceivably be deployed: it presents the "problem of abundance" in acute form. It includes studies of individuals, singly and in groups or masses, in specified official positions of leadership or in bureaucratic cadres; of administrative or technical procedures, like hearings or annual reports or traffic controls; of institutions of varying

degrees of complexity: i.e., administrative organizations; and of ideas, interests, opinions, and motives overtly manifested or inferred from behavior.

Despite this range, and the intellectual challenge of scientific discovery, scientific behavioral theory, both about individuals and about institutions, has been commonly disparaged, not only because it is "purely descriptive," but also because of two inherent limitations: the practical exclusion of the possibility of controlled experiments—the most fruitful tool of the natural scientists—in the real world, and the necessity of sticking to phenomena that can be counted in order to derive generalizations. Case study narratives can, in principle, be descriptively accurate and theoretically suggestive, but in their very nature they contain too many unique elements to prove a scientific generalization beyond the case.

The restrictive effects of these limitations, however, have been at least importantly relaxed by the recent introduction of a new analytical tool in the shape of the electronic computer, which enables the artificial simulation of some kinds of human behavior and opens up vistas of controlled experiments at the same time that it makes manageable more complex mathematical operations than Newton dreamed of. A neo-Taylorism called "operations research" is one product of this emancipation.

It is premature to estimate the full consequences of the technological revolution now going on in the methods of behavioral science. I would take my stand considerably short of Herbert Simon's confident optimism that in another twenty years we can substitute a computer program for the Supreme Court. I am not much alarmed either, for reasons to be explained presently, over the ominous prospects so vividly suggested by George Orwell's *1984*, by Aldous Huxley's *Brave New World*, and by Harold Lasswell's 1956 APSA presidential address,⁹ that scientific knowledge of behavior will bring a new human enslavement and even a new species of subhumanity. But I have no doubt that, to the extent that University bureau staffs learn to use computer techniques, they will find applications that open up to scientific study many phenomena now known only impressionistically, if at all.

No amount of behavioral knowledge, however, and no generalization from it, assures us an answer to the question, already noticed, of what administrative behavior to study. Not *all* behavior (even if that were possible) and not just *any* behavior, is significant. It is a common complaint that researchers, intent on being precise, too often count what can be counted only because it can be counted, and neglect what *needs*

⁹ See Harold D. Lasswell, "The Political Science of Science," *American Political Science Review* 50(4): 961–979 (December 1956).

to be studied. Here I will pass by the general question of what is significant by recalling Carl Becker's definition of an explanation as anything that causes an inquiring mind to come to rest. How various and subjective explanations may be is illustrated in Herring's anecdote:

A student who has just had three or four cocktails is shaving when his roommate slams the door. He drops his razor and cuts his toe, carelessly ties up the wound and goes to the theater. His toe becomes inflamed and he finally dies. What is the cause of his death?

The following causes, among others, may be assigned by various persons: General septicemia, by the doctor; a streptococcus infection, by the bacteriologist; there was no one to take proper care of him, by the mother; the doctor's incompetency, by the father; the slamming of the door, by the roommate; lack of discipline in the boy's upbringing, by the uncle; the boy's neglect of the wound, by a timid and also by a methodical friend; the cocktails, by a prohibitionist.¹⁰

But for research in administration the relevant behavior to study, the significant explanations to be sought, are selected for us by what we know of the functions of individuals and institutions and by generalizations about functions form a second type of theory, which may be called functional.¹¹ I use the term not in its mathematical but rather in its organic or mechanical sense, to refer to the part an institution performs in some larger process or system, or in the accomplishment of some ulterior ends. We speak, for example, of the theory of position classification or capital budgeting, of slum clearance or grants-in-aid. And mention of ends reminds us that this sort of theory not only takes for granted that people have purposes, that organizations have policies, and that institutions have functions, but it also postulates that these are the justification for their existence and hence give point to their study in preference to behavior and events unconnected with administrative functions.

There is a critical ambiguity in my usage of the term "function" up to this point. It may refer to the purposes that people have, or give, for their actions and utterances or for their institutions. These may be purposes existing in general popular understanding, or they may be conveyed, for example, in an organic law establishing an agency, or in policy pronouncements of officials. When they are "official" purposes, we mean that they have been arrived at or approved through some legitimizing process that endows them with a measure of authority. The approval

¹⁰ E. P. Herring, *The Politics of Democracy; American Parties in Action*, New York, W. W. Norton, 1940, p. 275.

¹¹ In this analysis I am much indebted to a paper on "Theory and Practice in the Study of Politics," prepared by my one-time colleague, Charles H. Wilson, now Principal of Leicester University, and read to the Conference of the Political Studies Association of the United Kingdom, March 1952.

process is ordinarily formal, but custom or acquiescence may serve as well. So we say the function of the Budget Bureau is to prepare and present the President's annual financial plan; the function of a hearing is to give those affected an opportunity to protest; the function of a governor's welcoming speech or tape-cutting ceremony is to give official notice and blessing to an event.

We may still use "function" for "purpose," having in mind not official purposes now but rather the private ends of anyone in an organization—or outside it—who has access to its facilities or influence on its activities, or who is affected by them. Evidently, these private (often group) purposes may correspond with, or run tangent or counter to, official purposes; in the latter case they give rise to what economists or system theorists call "dysfunctional" activities. So from the standpoint, say, of the appropriations committees, the function of the Budget Bureau may be to furnish needed papers on request and otherwise keep out of the way; in the view of the official reporter the hearing is just another pile of stenographic notes to be transcribed and to a group of children the governor's speech may be a chance for a half-holiday from school; these are all functions too.

But a function may be simply a consequence of an activity, as insomnia is of worry, or as planning uncertainties at some seasons are a consequence of an annual appropriations system, or inflexibility is a consequence of a line-item budget. So juvenile delinquency may be considered a function of overcrowded housing, and in this sense a major function of the Budget Bureau is the mediation of interdepartmental disputes; a function of a hearing is usually delay (regrettable or not) in action; and a function of the governor's ceremony is disruption of traffic. In this usage, evidently, purpose and function may or may not correspond. Some activities turn out as planned or desired; for others, a cause and effect can be discerned, but only *ex post facto*. In still other cases, unintended consequences go generally unperceived until some shrewd observer realizes how the system works, sees an opportunity for himself, and sets about turning the situation to his own advantage—thereby introducing a new purpose and setting another causal train in motion. A common example of such a situation is a "loophole" in a law.

In the study of administration, functional theory is still descriptive and hence scientific in form. It tells us what people want or expect from public officers, agencies, and processes, and what in fact they get. But descriptive statements of this sort, depending on their degree of generality, are considerably less rigorous than purely behavioral propositions: there are too many variables, the key terms are inexplicitly defined, and there is little chance of independent verification.

Take a simple illustration first. It is commonly said to be a function of political parties to develop political leadership and of elective offices to serve as proving grounds for candidates for higher responsibilities. There have not been so many mayors of New York City in the past half-century but that the careers of all of them can be studied. A perceptive student doing this noticed that none of them went on to higher office, and in a flash of insight from the observation that they went nowhere, leaped to the formulation of "Sayre's Law": Mayors of New York (ex-mayors, that is) have nowhere to go, politically speaking. This is functional theory; what is its standing in science? Is *La Guardia* an exception to be explained, or a clinching instance of proof? Is this law peculiar to New York City, like so many of the institutions of our largest metropolis?

Take now a more complex case. During World War II it was widely agreed that a central direction of industrial production and the allocation of resources was needed; the War Production Board was established for that purpose and it developed and operated priorities and a "controlled materials plan" to that end. Yet it is the unanimous verdict of observers that the WPB never succeeded in getting into the driver's seat as anticipated; and the result is often cited as an example of the maxim that "operations drive out staff work." This maxim is a bit of functional theory; what definitions, what evidence, what "research design" would be required to test it scientifically? Because it is indemonstrable, does it fall in that class of "proverbs" that Herbert Simon ridiculed?¹²

Two decades ago Pendleton Herring, in a well-known study of American politics, made a modest suggestion, equally applicable in the study of administration:

... that we consider as factors in the political equation: (a) individuals and the groups into which their common interests cluster them; (b) ideas, aspirations and philosophies which men accept for belief and guidance; (c) institutions and organizations which channel human behavior.¹³

Restated as the four I's—ideas, interests, individuals, and institutions—these have served as the theoretical framework for quantitative correlations in analyses of voting and other behavior. They have also guided the analysis and narrative in numerous case studies in public administration as well as such historical works as Leonard White's four volumes on federal administration. But consider the difficulties of applying them in an effort to test Sayre's Law outside New York, or the maxim about

¹² See Herbert A. Simon, *Administrative Behavior*, New York, Macmillan, 1947.

¹³ E. P. Herring, *op. cit.*, p. 26.

operations and staff work; they are "variables," to be sure, but each of them a whole basketful.

Functional theory, then, is dubious as science. But because it speaks of ends and means, of the purposes and consequences of activities, it has not only intrinsic interest but also two characteristic utilities. On the one hand it guides and informs behavioral research, whether that is strictly scientific, or journalistic or historical; it suggests the facts to be noticed as relevant, and the questions to be asked. So it is because of what we say about the functions of leadership in organizations, for example, that we turn to psychological studies of leadership roles;¹⁴ it is because of what we say about coordination that we examine the behavior of members of interdepartmental committees, or look into the mechanical details of intra organizational communications systems. On the other hand, functional theory yields the kinds of statements on which policy advice and judgments rest—generalizations that may be called "policy theory." Here the facts of intention and the facts of function are joined to make appraisals.

Policy theory is again descriptive in form, but if strictly stated it must necessarily be conditional, with a separate clause setting out explicitly the purposes and qualifications that limit the judgment expressed. It may be a judgment on past events, e.g., "If the purpose of the commission was to protect consumers against rate increases, its membership was badly chosen" (or, "its powers were inadequate," etc.) Or the theory may be an imperative for the future. So we might say "If it is desired to let the governor have his way with the budget, then give him an item veto," or "If you have to use force again in Little Rock, then don't send paratroopers," or "If you prefer speedy settlements to exact reckonings of public accounts, then don't insist on a procedure that requires an external audit of every voucher." A little reflection will show how far these particular examples are loose shorthand, how much more complicated an exact statement of all the conditions would have to be, and how far the "proof" for the imperatives depends on inference from historical cases rather than controlled verification.

Evidently, despite its form of statement, policy theory can scarcely claim to be within more than hailing distance of science. The degree of confidence to be accorded it in particular instances depends on at least three different kinds of considerations: the degree of generality attempted, the skill and judgment—the artistry—of the observer who formulates it, and the use to which it is put. The first of these is a lesson of

¹⁴ See, for example, the series of Ohio State Leadership Studies, under the direction of C. M. Shartle, published by the Bureau of Business Research, Ohio State University.

history about lessons of history.¹⁵ Nearly everything we have learned to do and teach derives in one way or another from experience, personal or vicarious. In public administration, so long as we stay within a familiar culture and a contemporary time span, we trust most in the continued efficacy of a repeatedly tried minor technological lesson in the form of a how-to-do-it device, like the audit of an account; we expect a wider range of applications, but only with a good deal of adaptation, from a more general problem-solving device like a budget system, for allocating time, money, and other things in short supply; and we expect only suggestion, rather than instruction, from a broad generalization such as that "consent is better than force." The second consideration is a recognition that administration in practice is as much art as applied science,¹⁶ and that perception and training lend authority even to hypothetical or conditional judgments in a field where the inexperienced go easily astray, and the most sophisticated have been known to err. The third is the elementary point that in the absence of sure knowledge citizens and officials who must act and decide will do so perforce on faith, hypothesis, or probability, though a scientist would wait for certainty before asserting a theory as a law.

Acknowledging these limitations, functional theory and policy theory are the goals of research that aim at understanding administration, whether that understanding is an end in itself, or a commodity to be purveyed through teaching, or a guide to purposeful action. This is a broad and rewarding field, encompassing most of the subjects and raising most of the questions that students and practitioners of public administration are likely to be interested in.

There is a fourth and final type of theory to distinguish here, that which treats of ends as good or bad, and not merely as well or ill adapted to secure stipulated ends; we may call it evaluative. It has two descriptive counterparts that can be more or less scientifically treated, one of which is the assembly and analysis of opinions historically held on the subject (e.g., by classical writers), and the other the collection and analysis by polling and statistical techniques of contemporary popular opinions about good and bad, better and worse, ends. But evaluative theory itself is philosophical, not scientific. It might tell us, as Walter Lippmann does, for instance, in *The Public Philosophy*, that the decline of the west is caused by the encroachment of legislatures on the rightful province of the executive, which prevents the best minds from ruling

¹⁵ See my essay, "The Uses of History," *Public Administration Review* 11(1): 51-57 (Winter 1951), where I have enlarged somewhat on this topic.

¹⁶ This appears to be the matter in dispute between Mosher and Honey. I think the dispute might evaporate if they took account of the different kinds of theory they are talking about.

unhindered by popular passions; or it might affirm a preference for democracy in administration as an end in itself.

Since administration is almost by definition regarded as a means rather than an end, it might be expected that evaluative theory would not loom large in the literature of the field, even if more practitioners and students were philosophers. Perhaps this is just as well, since it is difficult to establish what is good in the abstract and futile to discuss it very long without reference to what is feasible.

I have tried in this section to distinguish four types of theory that may emerge from research in public administration and to indicate broadly how far each of them may lay claims to being scientific. My main object has been clarification; since no complaint against the current state of that research is more common than bewailing its unscientific character, it seems useful to try to ascertain what parts of it may aspire to improvement in that regard and to suggest lines of defense for continuing the pursuit of some parts that may not.

An incidental object is to stake out the grounds for rejecting the prospect, held out so ominously by Orwell and Lasswell, that the advance of science may bring enslavement, not freedom. That prospect presupposes that some technocrats may gain such a mastery of behavioral and functional theory, as well as of the natural sciences, and such a lead over all other citizens and students, as to stand above them like *conquistadores* among Indians, armed with new and irresistible weapons, and so be able to reduce them to subjection and keep them there in accordance with a totalitarian policy theory. In contemplating this extension of Madison Avenue into upper Madison Avenue, we can take some comfort in the observation that Madison Avenue has not found the going all smooth here and now; a little diffusion of functional theory introduces counterpurposes all along the line.

My more immediately practical object is the improvement of all kinds of research in public administration. In any particular project several sorts of theory are apt to be mingled. It should aid in the design and execution of the project and in the interpretation of its results, if we are clear about the kind of inquiry we are pursuing, the kinds of evidence that are appropriate to it, and the kinds of results that may be expected from it.

Comment

THE ACADEMIC STUDENT of public administration has long exhibited a good deal of self-consciousness about his subject matter. Harvey Mansfield's essay continues a tradition at least as old as the work of the first Committee on Public Administration of the Social Science Research Council established in late 1928. Indeed, three stock-taking ventures identifying major intellectual issues as well as questions of strategy accompanied this committee's specific contributions to the definition of particular research problems. In 1930 John Gaus prepared for the committee, *A Study of Research in Public Administration*.¹ Fifteen years later William Anderson assessed the work of the Committee on Public Administration in a volume entitled, *Research in Public Administration*.² In this same volume John Gaus brought up to date his earlier report under the title, "Research in Public Administration, 1930-1945."

Falling between these two ventures in point of time came a third more particularized assessment of public administration under the committee. In 1937 the committee employed Frederick P. Gruenberg and Norman N. Gill to survey the governmental research movement. While the committee did not publish their report, William Anderson and Joseph Harris used it to identify issues that became the subject of vigorous debate at the September 1939 sessions of the Governmental Research Association.³

At least tentatively from the earlier appraisals and from Mansfield's analysis and those more recent studies he cites, we can articulate some persisting questions of strategy that university bureaus of public administration confront in directing their course. Gaus would infer from the characteristic conditions of our times some of the principal criteria for determining the priorities of possible researches in public administration and for deciding their relevance. Some twenty years ago the Governmental Research Association proposed to appraise studies of public administration by assessing their contribution to "citizen control" of government. For the critics of this point of view this seemed to mean

¹ See John M. Gaus, *A Study of Research in Public Administration*, New York, Social Science Research Council, 1930, Mimeo., 143 pp.

² William Anderson, *Research in Public Administration*, Chicago, Public Administration Service, 1945.

³ See *Governmental Research and Citizen Control of Government, Proceedings of the 28th Conference of the Governmental Research Association, Princeton, New Jersey, September 1939*, Detroit, Governmental Research Association, 1940.

shaping research in order to assist large taxpayers to enjoy the benefits of governmental "economy and efficiency." To provide another orientation these critics proposed the creation of what became late in 1939 the American Society for Public Administration, devoted to furthering the science and art of public administration on a broader front.

At the same 1939 meeting of the Governmental Research Association Charles A. Beard opened up another set of strategic questions around the possibility of developing a science of public administration. It is of course to this same area of analysis that Mansfield's essay makes its contribution. He may not make entirely clear whether he would work out the strategy of a bureau of public administration by measuring the way in which particular studies contributed to more rather than less scientific kinds of theory. Presumably the more scientific is the more valuable. Then a bureau may very well measure its program by the scale that puts behavioral theory at the top and evaluative theory at the bottom in relative contributions to the science of public administration.

Bearing directly on bureau strategy too are the questions Mansfield and the analysts of the governmental research movement raise with respect to the independence of the investigator of public administration. Hanging over bureaus presumably is the constant danger that their work become interested research rather than scholarship. Mansfield observes hopefully, "It is much less true now than two or three decades ago, and for a variety of reasons, that the horizons of state universities are defined by the state and local governments that form their original clienteles." But the threat to detached scholarship in public administration comes not only from politician, legislator, and administrative official. The businessman who supplies funds for the researcher may equally seek to shape the outcomes of investigation, and of course it is not inconceivable that the private foundation may equally have a stake in the outcomes of the studies of public affairs. Yet, rightly only the autonomous scholar can map the course of research in public administration with regard to the considerations of the highest demands of science. Only the autonomous scholar can maintain the values of the academic community as these are established in detached criticism of the world of affairs.

Yet we do not dispose of this question of the autonomy of the scholar by distinguishing between "pure" and "impure" research or by regarding as more valuable the scientific as opposed to the practical treatment of problems of public administration. With John Dewey we can repeat the general point he makes:

Our intelligence is bound up so far as its materials are concerned with

community life of which we are a part. We know what it communicates to us, and know according to the habits it forms in us.⁴

If, as Dewey notes, not even the mathematician nor physicist transcend the "existential matrix of inquiry," how much more will the student of public administration need to derive the questions he directs towards his subject matter from the perception of his time and setting.⁵ Rather than seeking scholarly autonomy through various kinds of withdrawal or through the creation of a variety of communications barriers with the world outside, the study of public administration may require the exploration of new relationships between the researcher and his field.

Because of the nature of public administration, inquiry may implicate the investigator in social action. As any investigator, presumably he also cannot avoid entering into his subject matter. If the very act of inquiry by the physicist determines what it is that he may find, if his investigation shapes his subject matter, certainly inquiry in public administration, will, by the very act of investigation, shape what is to be found. This is more than mere feed-back through the participants in the subject studied. In both subtle and direct ways, for example, in the very selection of a question for investigation, a bureau of public administration has modified the processes of public affairs. Consequently, its choice of strategy, its decisions about the priorities of its endeavors are not "autonomous" in the conventional sense, but are in a compelling way in the context of contemporary society and its problems.

It is often true that inquiries growing out of the work of a bureau of public administration involve its investigators in direct participation in public affairs. Many bureaus have sought indirectly to exercise influences as advisors and councilors of political decision-makers. Perhaps none have frankly faced the questions that might be raised if the investigator were *as a researcher* also to take a direct hand in public affairs. It is usually assumed that research and action remain in separate spheres. If a member of the staff of a bureau occasionally takes on some kind of public employment in the practice of administration, typically he takes leave or in some other way symbolically dissociates himself from his research enterprise. But another approach may be possible.

It may be that university bureaus of public administration can develop a strategy for meeting a need that Mary Parker Follett has identified:

We need then those who are frankly participant-observers, those who will try experiment after experiment and note results, experiments in making

⁴ John Dewey, *Human Nature and Conduct*, New York, Henry Holt, 1922.

⁵ See John Dewey, *Logic: The Theory of Inquiry*, New York, Henry Holt, 1938, Ch. XIX and pp. 505, 511.

human interplay productive—in industry and business, in legislative committees and administrative commissions, in trade unions and shop committees and joint boards of control, in athletic committees and college faculties, in our families, in parliamentary cabinets and international conferences.⁶

To conceive of a university bureau as integrally related to its community in this fashion may require us to assess some facts about the contemporary university. The life of the mind that centers in universities implies a threat to many who would capitalize on ignorance and fear in the world. For them a university is fundamentally subversive, for it constantly demonstrates that things are not as they seem, that many questions about our way of life remain to be answered, that criticism is a condition of progress.

Universities are also expensive. To many the results they produce, especially in the social sciences, hardly seem worth the cost. When the minds of men are plagued by fears and confusions, those who offer simple and direct solutions to our problems are often quite skeptical about the obscure and often seemingly irrelevant findings of men and women in the universities.

What may be said generally about universities may be said with particular force about the study of public administration. For it pertains to a wide range of activities designed to protect and to advance the values of groups of men and women engaged in cooperative endeavors under some more or less rational plan for the division of labor. Purposeful and sophisticated administration reflects our contemporary commitment to problem-solving. Our public administration gives testimony to the faith that men can plan to make their lives better, that they can invent arrangements of their affairs more suitable to their needs, that they can modify their own situations significantly in their own interests. Consequently, the study of administration promises to deal with matters intimately touching affairs in which citizens and officials have a large stake. To study these is both to extend a promise and to pose a threat to the community. To study these is then to create tensions that may seriously endanger the autonomy of the investigator. Can such tensions be resolved so that community and bureau of public administration work in partnership?

Partnership of course stresses autonomy of the participants engaged in a mutual undertaking. Partnership represents a division of labor in the solution of common problems. Policy-maker and researcher, while having different goals, nevertheless are inevitably connected in the treatment of common materials by virtue of the subject matter occupying

⁶ Mary Parker Follett, *Creative Experience*, New York, Longmans Green and Co., 1924.

their attention. Joint participant-observation may provide clues concerning the nature of the relationship between the investigator and his subjects. The community may challenge the bureau of public administration to participate in the affairs of people who conduct the business of our society. In its turn the university may challenge the community to approach the solution of its problems experimentally rather than on the basis of fixed dogma or superstitions. Both may work in the community as in a laboratory where problems are not contrived, but rather are treated in the regular course of policy-making as occasions for testing hypotheses in action.

Some fifty years ago Charles McCarthy and his colleagues at the University of Wisconsin evolved "the Wisconsin idea." It rested fundamentally on the notion that the university could realize its function of advancing knowledge as it gave men in high places access to the best that students of nature and society might produce. McCarthy had observed how German universities in the latter part of the nineteenth century served virtually as arms of the state. He believed that the university in the United States might contribute to its own development by actively helping to shape the policies of a democratic society. In practical terms, McCarthy linked the University of Wisconsin substantially to the fortunes of the progressive Republican group under the leadership of LaFollette.

To interpret McCarthy's own analysis, we might say that growth and development of university and community take place in their relationships with one another. McCarthy viewed learning as going on and intelligence as taking shape in the real situations with which men and women were grappling. He sensed what Mary Parker Follett points out, how the solutions of intellectual problems can be sought in the interplay of human minds.

With the university generally its bureau of public administration shares characteristics that make its relations to its community difficult. The university is a specialized institution where refinements of specialized knowledge constitute important achievement. A university is also primarily an educational enterprise necessarily detached from the world of affairs. As Mary Parker Follett puts it:

Our problem is to find a method by which the opinion of the expert does not coerce and yet enters integrally into the situation. Our problem is to find a way by which the specialist's kind of knowledge and the executive's kind of knowledge can be joined.⁷

In the community, too, rather than participant-observers we are apt

⁷ Mary Parker Follett, *Freedom and Coordination*, London, Management Publications Trust, 1949.

to find true believers. To experiment, to test a proposal, to prepare for revision in a course of action often repels rather than attracts people. If the "Wisconsin idea" was popular, it almost surely promised more than it could really deliver. The perplexing difficulties of our times often make men and women desperate and ready to accept a pat solution for their problems. Instead of partners in a search, they then become victims of a prescription. An absolutistic logic, fatal to freedom and destructive of democracy, often has so wide an appeal that its popularity is readily confused with rightness.

Even leaders in the community reject experimentation and criticism. They become virtually professional protagonists for their programs and pretend to offer their followers final and complete "answers" rather than "hypotheses for testing," in their policy proposals. Effective partnership between community and bureau of public administration becomes virtually impossible under such circumstances, since the conditions of university work require an experimental approach and an attitude of criticism.⁸

The policy-makers are the doers, the practical people of affairs. University people are critics, the men and women engaged in theoretical speculative enterprises. Both are indispensable for the solution of society's problems. Each needs the corrective, as we might call it, of the other's functions. Policy-makers at various levels of government, in business, in labor unions, in community enterprises, in the practice of law, confront and dispose of lively issues and their decisions reach out to shape what we still regard as private affairs. From the wide range of activities and relationships of policy-makers the students of public administration can draw inspiration and insights. To these activities and relationships they can bring the products of their scholarship. Their researches can illuminate questions of public policy as their teaching helps to prepare leaders for policy making.

In the broadest and most general sense, public policy can serve the social scientist as hypothesis. The administration of public policy can represent a testing of hypothesis. Consequently the student of society can find, in effect, a laboratory in the policy process. And a bureau of public administration might aim to create conditions favorable to scholarly work by giving a university's researchers an opportunity to work in the only laboratory where policy is made and tested, namely in institutions and arrangements for governing our society.

So the strategy of programming the work of a bureau of public administration takes shape in the community. The academic scholar

⁸ John Dewey, *The Public and Its Problems* (New York: Henry Holt, 1927) elaborates this point, especially in Ch. VI.

has developed methods of observation and criticism and these may serve the policy-maker to develop an experimental approach to his decisions. But to share his method with policy-makers and to help them equip themselves for the function of criticism requires the scholar to convert theoretical knowledge into active intelligence. On the university bureau falls the burden of resolving conflicts between theory and practice. The bureau must invent bridges between theory and practice: every theoretical proposition must become testable in the lives and activities of the people and, simultaneously, every policy must come to be viewed as subject to the criticism that theoretical enterprises can provide.

Study and action would no longer be discrete enterprises. We may distinguish what we might call "areas of ignorance" to which study should first be given, but implicit in this approach to the strategy of research in public administration is action to deal with our ignorance in the course of policy-making and criticism.

This implies too that the strategy of research will aim to illuminate the conditions of policy-making both for policy-makers and researchers in public administration. Joint systematic study, for example, through methods of participant-observation of problems long identified as critical in our industrial society, may help to focus the products of research upon such a goal as training a "generalizing mind" of the kind that Brooks Adams, Mary Parker Follett, and Elton Mayo, among others, have demanded for our time. What such a mind might consist in can probably be inferred only from continuing study of the conditions of decision-making. Once these are more clearly seen, researcher and policy-maker can begin to define the knowledges and skills required to develop a base for a research program.

What our policy-makers decide affects and in turn is affected by the physical and human situation involved in their decisions. The theory of these mutual interrelations is reasonably clear, but joint participant-observation is required to make what John Gaus has usefully called "the ecology of government"⁹ a working part of the equipment of both policy-makers and social scientists. Typically, the academic researcher knows or can find out a great deal about the physical and human situation involved in the decisions of policy-makers. The historical study of economic or political or religious values is likewise part of the traditional subject of study in universities and has significance for the goals that policy-makers settle on. But how to make this actual or potential knowledge relevant for policy-makers is a fundamental and

⁹ See John M. Gaus, *Reflections of Public Administration*, University, University of Alabama Press, 1946, Ch. 1.

inescapable strategic question to which the researcher in public administration has to give attention because of the nature of the subject with which he deals. His treatment of Robert Lynd's question "knowledge for what?"¹⁰ is peculiarly related to participation in public affairs.

Can university bureaus provide opportunities for direct participation in public administration within such a framework? Can they maintain autonomous scholarship while deeply implicated in public affairs? Agencies of government often use members of university faculties as advisors; as members of interim committees, boards, and commissions; and occasionally as holders of elective offices, especially in local governments. As Mansfield notes, in the generalized field of public administration large numbers of faculty also work on assignments abroad. In these varied relationships, the scholar contributes his intelligence and criticism to the processes of public affairs. He draws on his associations with full-time policy-makers for heightened insights into the nature of problems worthy of his deliberate study.

A bureau of public administration could also contribute new arrangements in its university and strengthen conditions hospitable to scholarly advances in public administration. To bring the policy-maker and the student of administration together in a relationship of participant-observation is a difficult enough task, but the strategy of research in public administration may also confront a university bureau with the challenge of relating more of the university's total intelligence to the large issues confronting our society. So, research in public administration includes a kind of brokerage function for a bureau, a function incidentally implied in the fact that bureaus are often closely connected with regular university departments. Some of the arrangements might aid to ignite the creative spark in the solitary searcher for truth. Others might encourage groups of scholars, a research team, or interdisciplinary seminar to carry forward studies of public policy. Still others may enable the industry and enthusiasm of a group of students to complement the genius of the mature scholar. The bureau might invite appropriate scholars to pursue solutions to problems already formulated by policy-makers. The bureau might at the same time find ways to encourage scholars to name and study problems whose relevance and importance only they see clearly. In all of this the bureau functions to offer university faculty people access to public affairs and policy-makers access to the intelligence of the university community. This may be included in what Mary Parker Follett calls "experiments in making human interplay productive."

¹⁰ See Robert S. Lynd, *Knowledge for What?*, Princeton, Princeton University Press, 1939.

All of what has been suggested here must remain most tentative. To evolve a research strategy for a bureau of public administration is a concrete task and the character of the strategy derives from the quality of the university as a scholarly center carrying on research, teaching, and public service. It is clear that live issues of public policy may challenge the investigator in public administration to find new methods to join theory and practice in creative relationships. Here may be made some beginnings in the development of a science without some of the invidious overtones of grading various kinds of intellectual pursuits according to their conformity to a model whose relevance may be questioned. We have no laboratory with its control of variables; but quite possibly the student of public administration can invent procedures and methods around such a concept as participant-observation, simultaneously satisfying the objectives of scientific research and of intelligent social action.¹¹ Thus bureaus of public administration in universities may become centers for the development of a new "science" of administration, building on knowledge (both methodological and procedural) derived from such experimentation.

¹¹ See Charles E. Lindblom, "The Science of 'Muddling Through,'" *Public Administration Review* 19(2): 79-88 (Spring 1959) for some suggestions for a science of public administration that uses models other than those of laboratory science.

Research in Politics

BEHAVIORAL ORIENTATION

I find it difficult to establish any reasonable limits to a topic as vague and inviting as "Research in Politics." If politics is a continuous exercise in the logic of ambiguity, it is both fitting and proper that any discussion of research in this field be carried on under a rubric as ambiguous as possible. Certainly "research in politics" meets this test.

Later I should like to explain why I believe politics can be described as a "continuous exercise in the logic of ambiguity." But first some words by way of orientation.

Since I regard politics as a special type of human behavior, I place myself, I suppose, in a camp with the so-called behavioralists even though I dislike the term. Certainly if politics is not human behavior, it's hard to know what it is.

Critics of the so-called Behaviorist School have objected, and I believe rightly so, to a good many extreme, narrow, and dogmatic statements made by some members of this school. They have objected to their preoccupation with *action*, and with microsociology, model-building, quantification, symbolic logic, and their passion for neologistic polysyllabism (gobbledygook) that defies definition or translation. The critics have objected also to the arrogant solipsism of some so-called behavioralists who see all political and social science in their own image and regard those outside their own narrow guild as fuzzy-minded taxonomists, mystical idealists, or pseudo-scientists. Most of all they object to the alleged neglect by many behavioralists of the historical, institutional, legal, normative, and philosophical problems with which political scientists from Aristotle to Arnold Brecht, and political practitioners from Pericles to "Soapy" Williams, have been concerned.

Many of these criticisms are founded on isolated instances of behavioralistic misbehavior by bad-mannered, or ill-tempered, young scholars who fancy themselves as pioneers on a new scientific frontier being shot at from ambush, and from behind, by tribes of reactionary pedants. Some of the criticisms are born of ignorance of what the behaviorist is trying to do and of the very real relevance of behavioral studies—even model-building and microsociology—to the persistent

problems of political science. Some critics do not even try to understand any terminology but their own. Other criticisms represent rationalizations of vested academic interests threatened, they think, by an invasion of young barbarians calling themselves behavioralists. Our profession has experienced invasions before and has been vitalized and enriched by them. I am inclined, however, to believe that most critics of behavioralism simply fail to realize the scope of scholarly interest and research that the study of human behavior and, more particularly, political behavior, affords. They forget that history is but a record of human behavior. Institutions, laws, and customs are made by and for men, not the other way around, and they can have little meaning or significance unless in some way they affect human thought and behavior. Moreover, ideas, systems of value, and scientific and philosophical theories are products of human thought—which is itself a form of behavior—and have relevance and significance only as they affect or help us to understand human behavior.

Not the least important among these ideas for the student of politics are those theories of human nature and politics which serve to rationalize various systems of status and power or certain modes of political thought and behavior. It is, I believe, no accident that political conservatives as well as most nondemocratic or antidemocratic theorists take a dim view of human nature. To Thrasymachus, Machiavelli (at least in *The Prince*), Thomas Hobbes, Edmund Burke, Joseph De Maistre, Alexander Hamilton, Cardinal Newman, Mussolini, Oswald Spengler, and Hitler (to mention but a few) man was essentially a “beast of prey”—aggressive, nonrational, and antisocial. Only a strong ruler armed with a monopoly of violence and a will to use it against nonconformists and rebels could tame the beast and confine his private aggressions within tolerable limits. Time and tradition, custom and convention might help to develop, rationalize, and reinforce patterns of habitual obedience. But when these failed, there remained the power of organized force and violence—naked, absolute, and—as the behavioralists would say—asymmetrical. There is little room in these theories for such notions as the malleability or perfectability of human nature, for mass participation in decision-making, or for rationally induced changes in social or political systems. Social classes, power structures, patterns of income and deference, according to this view, are rooted in instinct and heredity, inevitable and immutable, save as God and Nature might contrive to alter or abolish them. This Hobbesian-Spenglerian view of human nature, usually if not invariably, seems to accompany attitudes of cynicism, ethnocentrism, disillusionment, resentment, insecurity, and a general hostility to both rationality and am-

biguity. Present-day conservative and antidemocratic opinion continues to cling to this pejorative-pessimistic theory of human nature. Adorno, Frenkel-Brunswick, Levinson, and Sanford's study of the *Authoritarian Personality* and Herbert McClosky's monograph on *Conservatism and Personality*, among others, tend to confirm the integral relation between theories of human nature and political attitudes and behavior.

Opposed to the conservative antidemocratic theories of human depravity are those that see man not as a beast of prey but primarily as a social animal—rational and cooperative—inspired by impulses of mutual aid, love, and compassion. In general this view of human nature provides the psychological basis for most liberal and democratic theories of politics. Locke, Rousseau, Richard Hooker, Diderot, and Jefferson in varying degrees espoused this melioristic-optimistic theory of human nature. It furnishes a behavioralistic rationalization for belief in the possibility of human perfectability, mass participation in decision-making, and the possibility for rational adaptations of social and political institutions to changing human needs. It usually accompanies attitudes of optimism, security, racial and religious toleration, an abiding faith in rationality, and a high degree of toleration for ambiguity, i.e., for politics.

Most contemporary students of political behavior reject these Manichean theories which see human nature as either good or evil, rational or nonrational, social or antisocial. They also realize that to understand politics it is not enough to know that theories of human nature and of politics are interdependent. To say that one's political attitudes and behavior are due to his notions about human nature and conduct in general tells us no more and makes no more sense than to say that one's notions about human nature are due to one's political attitudes. Which is cause and which effect—indeed, if any causal relation exists at all—is by no means clear. And anyway, what we really want to know is why people have different notions about these important matters. Why do some become liberals and others conservatives? Why are some Republicans or Democrats and others Socialists, Fascists, or Communists? Unless we are to make the impossible assumption that men get such attitudes in the germ plasm, we need to ask how they do come by them and why they change from time to time. Answers to these questions may also help in understanding other aspects of political behavior in the legislative, administrative, executive, and judicial process.

The study of politics in these terms has had to wait upon the development of a social science in which the systematic study of human attitudes and behavior is at least as important as the study of the formal

structure of institutions and the history and analysis of social and political thought. Indeed, only when political institutions and political philosophy *are* related to political behavior, can we hope to understand politics as a dynamic force in any society.

But this in turn has had to wait upon the development of a theory of human nature, more hospitable to empirical and scientific inquiry than the Manichean theories of the seventeenth- and eighteenth-century philosopher-psychologist. So long as the study of human nature was carried on solely in such terms, so long as it assumed that mind and body were distinct and unrelated phenomena, so long as it relied solely or mainly on the method of introspection, and so long as it was committed to the rationalistic methodology of the Enlightenment, the study of politics as a behavioral science was impossible.

This is not the place to trace in detail the transition from these time-honored concepts and methods to those of contemporary social science. To do so would require analysis of the radical determinism and associationism of Hobbes, Locke, and Hume—and the contributions of many physiologists and psychologists in resolving the old mind-body problem, and in substituting objective observation and measurement for imagination and introspection in the study of human nature. Above all, such a review would require mention of Ivan Petrovitch Pavlov and John B. Watson, who made the familiar S-R formula not only a scientific symbol but a popular slogan. The radical behaviorist had no use for concepts like the unconscious, instinct, or even consciousness and reason, except as these could be described in purely behavioral terms. Except for purely physical or biological traits, individual and group deficiencies were explained by “conditioning.” Not heredity but environment, i.e., the conditioning of the human organism by external stimuli, sufficed to explain differences of character and personality, attitudes, ideas, and behavior patterns.

By 1915 the so-called behaviorist school included most psychologists and was making converts in other social sciences. In 1933 Wesley Mitchell, the economist, told a social science conference at Chicago: “An obvious basis for integration [in the social sciences] is provided by the observation that all social sciences are concerned with the same thing—human behavior.” By the end of World War II, Mitchell’s suggestion had found wide acceptance, and when the Ford Foundation established its Division of Behavioral Sciences the term became all but synonymous with what old-timers remember as the social sciences.

My few words of orientation have already grown to several pages—yet something more needs to be said if “research in politics” as the systematic study of political behavior is to be understood. The initial

concepts and methods of the radical behaviorists have been profoundly modified and enriched since the robust and raucous voice of John Watson moved from Johns Hopkins to Madison Avenue. The simple S-R formula—a sort of nickel-in-the-slot-machine psychology—broke down in the face of empirical and experimental evidence showing the extreme complexity and subtlety of the conditioning process. So subtle and complex are the human personality and its external environment that it is difficult if not impossible to factor out any single item as *the* stimulus, or to say that the observable and measurable reaction to any stimulus comprises the total behavior of the individual in any given situation. It has long been apparent to most psychologists that behavior, as Edward Tolman has said, “is [something] more than and different from the sum of its physiological parts.” In place of the simple molecular theory of stimulus and response, Tolman and others advance a *molar* theory of behavior as an “emergent phenomenon that has descriptive and definite properties of its own.” Integration of one act with another and with emotional experiences outside the field of immediate perception is involved.

As Wolfgang Köhler puts it:

... instead of reacting to local stimuli by local and mutually independent events, the organism responds to the pattern of stimuli to which it is exposed, [and] this ... is a unitary process, a functional whole, which gives in experience a sensory scene rather than a mosaic of local sensations. ... A man's actions are commonly related to a well-structured field. ... The stimulus-response formula ... ignores the fact that between the stimuli and response there occurs the processes of organization, particularly the formulation of group units in which parts acquire new characteristics.¹

That is to say, within the total “life space” of the individual there is what Kurt Lewin has called a “psychological environment,” or field, more directly related to his present needs and interests. His response to objects or events within this field may be positive or negative (i.e., what Lewin called a positive or negative “valence”) depending upon whether they promise to meet his needs or threaten injury. These positive or negative forces (or vectors) produce action of approach or withdrawal; approval or rejection of objects, events, or propositions within the psychological field. Where two or more vectors operate at the same time in the same direction, they tend to reinforce one another and to produce a more emphatic response in a given direction. Where vectors are in conflict or operate at cross purposes, the result may be a weakened response in any given direction, or frustration, tension, or apathy.

¹ Wolfgang Köhler, *Gestalt Psychology*, Rev. Ed., New York, H. Liveright, 1947.

This process of organization is normally accompanied by the selection of some part or parts of the total pattern for special attention or emphasis. The patterns or configurations to which the individual responds and the particular objects or events to which he gives attention are themselves determined not only by their physical properties of proximity, similarity, or continuity, but by the life history of the individual. Thus, different individuals respond differently to the same or similar situations depending on their background, education, and experience. Few psychologists now believe that this process of organization, selection, and response is altogether or always unconscious and nonrational. Indeed consciousness, recognition, and recall, even reason, value, and purpose play a larger role in the process than many of the more radical behaviorists were willing to admit. Moreover, if rationality is regarded not as some mystical daemon or faculty which some people inherit and others do not, but as a mode of conditioned behavior, it is not too fanciful to hope that reason may play an ever-increasing role in human behavior.

Contemporary students of behavior no longer try to explain everything in terms of a single construct or formula such as association, instinct, gestalt, or S-R. There is also a renewed interest in research on motivation and purpose, as developed by Freud and his successors for whom the Watsonians had only scorn. The principle of psychic determination underlying modern psychoanalytical theory is now generally accepted if only as an inarticulate major premise. So, too, are such basic Freudian concepts as "instinctual drives" or impulses, "inhibition" or "frustration," and even the "unconscious." The tripartite description of behavior in such terms as the Id, Superego, and Ego—or Instinct, Conscience, and Rationality—has wide acceptance. And what Freud called "defense mechanisms," i.e., compensation, projection, and rationalization, are now common coin in behavioral research. Even the so-called Oedipus problem to which psychoanalysts have devoted so much attention crops up in current discussions of the "father image" as a factor in describing the relations of leaders and their followers.

Contemporary students of behavior take issue not with Freud's basic principles or concepts but with his excessive emphasis on infantile sexuality to the neglect of external factors in the individual's psychological field. They quarrel especially with his neglect of the important role of the family, the school, the church, and other social and cultural factors. This renewed emphasis upon man as a social animal is reflected also in the important contributions made by sociology and social psychology to research on human behavior including political behavior.

This is not the place to review, even by mere reference, the extensive literature in these fields. Studies of group behavior, both theoretical and empirical, of attitudes (structured and unstructured, central and peripheral), and of public opinion and communication, have special relevance for students of political behavior.

I should personally like to see more in the way of critical examination of some of the propositions and hypotheses advanced by our colleagues in these fields. What, for example, is the nature and relative importance of so-called structured and non-structured attitudes in understanding politics? To what extent are structured attitudes subject to change as education increases? What role, if any, does imitation, as this term was used by Walter Bagehot, Le Bon, and Tarde, play in politics? Has anyone undertaken seriously to inquire into the special relevance of Sumner's *Folkways* for contemporary American politics? Do Pareto's *residues* have any meaning or significance for students of political behavior? What empirical evidence do we have to show that Freud's genotypes (oral-erotic, anal-retentive-erotic, genito-erotic) have any political significance? Do the typologies of W. I. Thomas (the philistine, the bohemian, and the creative personality); Kretschmer (pyknic, asthenic, etc.); Sheldon (endomorph, ectomorph, mesomorph); Pareto (rentier and speculator); Lowell (sanguine, nonsanguine); Riesman (tradition, inner- and other-directed); and others throw significant light on political behavior?

Have the extraordinary insights of Max Weber's essay on "Politics as a Vocation" and the "Sociology of Charismatic Authority" had the kind of systematic analysis that his essays on "Class, Status, and Party," and on "Bureaucracy," have had? Have the theories of "interaction," "equilibrium," "power," "cohesiveness," "common fate," etc., as used by Homans, Festinger, Yule, Parsons, Easton, and Campbell been tested sufficiently to give them a prudential value in understanding political behavior or a practical value in political organization and management? Do they help us to understand legislative committees, for example, or party committees, let alone the NAACP or the AMA? What in general are the implications of contemporary group theory for those engaged in "research in politics?" Does the Group Theory of Politics give an accurate account of the political process? Is it compatible with theories of geographic representation, the single-member district, the party system, majority rule, the general welfare, individual freedom, and rationality? If, as Talcott Parsons argues, a social system is composed not of human individuals but of specialized and impersonal roles, what does this imply for democratic theory? If the mail-carrier, or the professor, or any other individual is, as Parsons says, "not a

person, just a role," what happens to the individual person who has been traditionally the center and focus not only of democratic theory but of the whole Judeo-Christian ethic? If, as Chester Barnard says, "organization action" is characterized by rationality to a "superlative degree . . . as contrasted with individual action," what happens to the commonly accepted notions of political scientists from Graham Wallas to Harold Lasswell that political behavior is essentially nonrational? Or is political behavior not regarded as *organization* behavior? If it is, are *political* behavior and *administrative* behavior to be distinguished by the degree of rationality involved? Are politics and administration to be distinguished by the degree of ambiguity which characterize the propositions or situations with which they deal?

One final point needs to be made in this orientation. This has to do with method and methodology. Every social scientist suffers from the initial hazard of being both observer and observed, both subject and object. As such, he is forever in danger of having his science obscured or biased by unconscious motives, stereotypes, value systems, prejudices, and predilections, of great strength and variety. As Henri Pirenne has said of history, every social or behavioral science is a subjective science. The behavioral scientist is further handicapped by the limitations of controlled experimentation in the study of social and political problems. His semantic tools are incredibly crude and ambiguous, and he lacks both the objective standards and the scientific instruments available to the natural scientist. W. W. Gring's book on *Laboratory Instrumentation in Psychology* has no counterpart in political science.

These hazards and handicaps no doubt help to explain the preoccupation of many behavioral scientists with problems of methodology. So much time and energy indeed have gone into methodological research that one is tempted to say, as Freud once said to his colleagues, "There comes a time when you ought to stop cleaning your spectacles and take a look through them." Unfortunately our scientific spectacles are almost hopelessly inadequate for the job we have to do. And this may be why so many of our colleagues have given themselves up to model-building, game theory, and recondite mathematical formulations of various kinds. As a Harvard psychologist observed, the mathematical model "seems to serve much like the philosopher's stone and leads people to forget about the phenomena themselves in their concern with elegant systems and whether they can fit into them." Model-building can, of course, help immensely to clarify the terms and methods of scientific inquiry. But it can also throw up roadblocks to effective communication among scholars. In extreme cases preoccupation with methodological exegesis makes otherwise able scientists in-

different to or unfit for the kind of empirical and applied research to which political scientists, at any rate, must continue to devote their major energies.

These reservations concerning the use of theoretical models and games theory in the social sciences are not meant to apply to mathematics as an instrument of scientific research. Historically, statistics—as the term itself indicates—was a by-product and instrument of politics, arising from the need of statesmen to estimate the military, fiscal, and material resources of the state. From the Domesday Survey of William the Conqueror in 1086 to the most recent U. S. Census, quantitative reports of various kinds have been indispensable tools of statecraft. In fact, it is from politics that statistical methods have been extended to other fields and not, as some critics seem to assume, the other way around. And it is mainly to the statistical services of the state that business and industry, labor and agriculture, economics and sociology, even history and political science, turn for the quantitative materials upon which they base their own scientific studies. Increasing skill and sophistication in the use of these materials has improved the quality of both politics and administration, in both theory and practice. The development of improved sampling, better methods of interviewing, and increased skill in coding and analyzing survey data have added a new and exciting dimension to social science research in nearly every field. New windows are being opened, through refined sampling techniques, upon many of the most difficult and complex problems of man in society.

An impressive catalogue could be compiled of significant studies in which quantitative methods of considerable scope and subtlety have been employed with great success. One thinks, for example, of various studies by Holcombe and Key, Lazarsfeld and Berelson, Angus Campbell, Warren Miller, James March, Robert Dahl, and David Truman, to name but a few in political science. If some of the contributions to Martin Shubik's *Readings in Game Theory and Political Behavior* strike one as recondite, they illustrate how far this type of analysis has advanced, at least on the theoretical level, since Stuart Rice's pioneering volume on *Quantitative Methods in Politics*.

So much, then, by way of orientation. May I now look a bit more closely at some aspects of political behavior as a frame of reference for "research in politics."

POLITICAL BEHAVIOR

Politics, viewed as a special kind of human behavior, is subject to the same incredibly complex array of influences, motives, and interests

that characterize other types of behavior. Normal human behavior, whether it be political, economic, religious, social, or sexual, is *goal* directed. It seeks satisfaction not only for basic physical needs (food, clothing, shelter) but for "psychic values" that derive from the interaction of human nature with the physical, social, and psychological environment in which men live. Among these psychic values are those described by the late W. I. Thomas as a desire for *new experience*, for *security*, for *recognition*, and for *response*. Others would include a striving for *deference*, *income*, and *safety*. And it may well be true that when Aristotle said "man is a political animal," he meant to say also that "man is a power-seeking animal." Thomas Hobbes certainly believed this. "I put (in the first place)," he said, "a general inclination of all mankind, a perpetual and restless desire of Power after Power, that ceaseth only in Death."

To establish any rank order of priority among these basic goals would require more knowledge of human nature than we now have. Nor is it possible to speak of goal-directed behavior as moving in separate and distinct channels or with equal force in all individuals everywhere and at all times. According to William James, the human striving for recognition and response, for self-esteem, personal significance, and prestige is the deepest principle of human behavior. Sigmund Freud regarded sexual goals as central to any real understanding of human nature and conduct; the acolytes and apostles of Karl Marx give top priority to economic goals; Thomas Hobbes put the desire for power in first place. Not least among the goals of human behavior is the continuous search for understanding both through the testing of reality, which is the goal of science, and through the elaboration of mythologies, moral codes, and religious systems, which are the products of rationalization. Only a brave but foolish man would undertake to say which of these is paramount among the varieties of goal-directed behavior which encompass and give meaning and purpose to human life.

The multiple goals for which men strive, moreover, are interrelated and interdependent to a degree that makes it all but impossible to disentangle one from another as the "efficient cause" of any particular act or any particular type of behavior. To speak of economic, or political, or sexual behavior as separate or distinct from one another or from behavior calculated to satisfy one's hunger for self-esteem, or prestige, or power, is to indulge in ghostly abstractions having little or no relation to actual human experience. Even when, for methodological purposes, we create ideal models of the "economic man," or the "political man," we run the risk of conserving the form by sacrificing the sub-

stance of scientific discourse. Economic goals are not confined to material goods, nor to something called "economic security"—but may include a striving for self-esteem, for power and prestige as well. Similarly, political goals may include not merely power, influence, and authority but also, as Harold Lasswell says, *income*, *deference*, and *safety*. And who is to say that all human goal-directed behavior is not in final analysis a striving for love and affection? In any case, as we have indicated, human behavior is *molar* not *molecular*—and covers, often at the same time, a wide range of *means* and *ends*.

Nor is it always easy to distinguish means from ends, i.e., to distinguish the *goals* of human behavior from the particular patterns of behavior by which men pursue these goals. Thus one may seek economic goals by seeking public office or by a campaign of political pressure and persuasion—just as another may seek political power and influence through economic channels. And the goals of self-esteem and prestige, love and affection, may be sought by behavior as varied as self-immolation or aggressive self-aggrandizement.

Is it, then, impossible to speak of political behavior as distinguished from other forms of behavior in any meaningful way? Are we to say that to understand politics one must perforce understand economics, and sociology, and physiology, and psychology, not to mention philosophy and all the other sciences devoted to the study of man? Nothing short of this, I am afraid, will suffice if one is *fully* to understand political behavior or, for that matter, any other type of goal-directed human activity.

Nevertheless, I believe it is possible to speak meaningfully of political behavior, and indeed to set forth at least the dim outlines of a program of "research in politics" if one does so knowing that we see the scene "as through a glass darkly." Any distinction between political behavior and other forms of human behavior must at best be fuzzy, ambiguous, and obscure. (There are no white or yellow lines on the highway to keep "political" traffic in a separate lane of its own.) We can, for example, say that political behavior is *power-oriented*, although *power* may be sought not as an end in itself but as a means to the realization of other goals, i.e., *income*, *deference*, and *safety*. And power may be achieved not only in the political state, but through membership in or control of a trade union, corporation, church, or other organized group or institution. One can therefore reasonably speak of politics as a characteristic mode of behavior in any group or institution in which some have power over others. Hence it is quite proper to speak of the politics of trade unions, churches, corporations, and even colleges and universities. Indeed one can learn a good deal about political behavior from

systematic study of these so-called voluntary associations. It is, however, hazardous to assume that what may be true of the internal politics of voluntary groups will be equally true of the politics of the State of California or the United States. It is not only that membership in the political state is "involuntary," or that its jurisdiction and authority are more comprehensive than other associations—but the sanctions by which its commands are enforced are vastly more compelling and conclusive. Only the political state may legitimately use physical force to insure compliance with its commands.

Political behavior is not only power oriented—it has to do with *public* rather than *private* affairs. This distinction is again as much one of degree as of kind for, as John Dewey has said in *The Public and Its Problems*, the essence of public, as distinguished from private, affairs is "the perception of consequences which are projected in important ways beyond the persons and associations directly concerned in them." And public affairs become "political" when special agencies are established to care for them. "Politics" involves all behavior that is directed toward their control and management. Because the jurisdiction and authority of the political state is more comprehensive and compelling than that of any other association, because it embraces a wider "public," its political life is more vital and more important than the politics of other groups and associations most of which are subordinate to it.

Finally, one may say that political behavior involves choosing among alternative goals and the means for achieving them. That is to say, politics is a decision-making process concerning the "public affairs" of any organized group or association.

But not all decision-making is political. The decisions of individuals or of groups in the market place—choosing among a variety of economic goods or values—may have political consequences but are not themselves "political." Nor are other decisions concerning domestic relations, religious belief or affiliation, or countless others having to do with personal conduct or inter-personal relations. Politics is a process for making decisions concerning the "public affairs" of any organized group or association through "special agencies," i.e., governments established for this purpose. And the "politics" of the political state are unique in that its decisions may be enforced by sanctions that include physical force.

Decision-making is another name for voting—for when one chooses among alternatives, whether of people or policies, he votes for one rather than another. Political behavior therefore includes voting behavior. But not all voting behavior is political. When one chooses, i.e., decides, among a number of competing items in the market place or

when one buys a theater ticket for one play rather than another, one is voting even though no formal ballots are cast. Such informal "elections" may even have political consequences without themselves being political. When an individual makes a choice or a decision, i.e., casts his vote in the market place in a competitive price system, he chooses only for himself or his friends and family. When on the other hand he casts his vote for an officer or representative in the government of a group or association, he must perforce think of a "public," whether large or small. And when he votes for President of the United States, his choice will affect not only himself and perhaps his narrow circle of friends and acquaintances but the whole of American society. To the degree therefore that his decision, or his vote, affects an organized public, to that extent is it political.² Bruno Leoni writes:

Robinson Crusoe politics do not make sense, although Robinson Crusoe economics may be . . . consistently conceived. Crusoe made individual choices, but he could not participate in group decisions (i.e., politics) until Friday came to join him. . . . In market choice the individual is the choosing entity as well as the entity for which the choices are made; whereas in voting (at least in what we usually call political voting) while the individual is the acting or choosing entity, the collectivity is the entity for which decisions are made.³

If we are to understand politics, therefore, we need to inquire into the forces that affect—or, as the psychologists say, "condition"—political behavior. "Human nature," someone has said, "is an intensely private and personal thing." Each man sees and feels the world about him from his own frame of reference and in terms of his own personal striving for self-esteem, security, power, or other goals that give meaning and direction to his life. Political behavior is but one expression of his goal-seeking—one means for achieving satisfaction for his private wishes and desires. But unlike, or at least to a greater degree than, his behavior in the market place, in church, or in the bosom of his family, politics involves "publics" of varying dimensions and complexity. Thus we may say that political behavior represents the expression of private wishes or private fears in terms of public persons or policies and in a public forum. Or, as Professor Lasswell has argued, politics is the "displacement of private affects upon public objects." And these "private affects" may be as simple, obvious, and self-conscious as the desire for a job on a public payroll or as complex, obscure, and unconscious as an Oedipus complex. All human striving arises from a sense of in-

² See J. M. Buchanan, "Individual Choice in Voting and the Market," *Journal of Political Economy* 63: 334-343 (August 1954).

³ Bruno Leoni, "The Meaning of 'Political' in Political Decisions," *Political Studies* 5: 225-239 (October 1957).

dividual privation or helplessness in the face of an often hostile environment. As Freud has said:

... any given process originates in an unpleasant state of tension (or conflict) and thereupon determines for itself such a path that the ultimate issue coincides with a relaxation of this tension.⁴

Political behavior is no exception. If political behavior is power-oriented, it is because in the struggle for power, or in the exercise of power, by himself or others, the individual not only hopes to find satisfaction for his private wishes but also relaxation for his own inner tensions or conflicts. Thus, as Karen Horney observes:

... striving for power serves... as a protection against helplessness which... is one of the basic elements in anxiety. In the second place, a striving for power serves as a protection against the danger of feeling or being regarded as insignificant.⁵

The more one feels frustrated and helpless, the more does he wish to avoid the appearance of weakness or inadequacy. To avoid these "dangers," one strives for identification with the successful and the strong, a process that, within the family, may result in a father fixation. In the political world it helps to explain the appeal of the strong charismatic leader to millions of voters who find in him a father image. As Patrick Mullahy says:

The father's perfection of power and his untrammelled freedom grew (in time) into an ideal capturing the minds of men, with a willingness to submit themselves to him. . . . Certain individuals who had distinguished themselves above all others came to be invested with the qualities of the father's ideal.⁶

Impelled by inexorable demands for physical and psychic values—and confronted in his struggle for realization by obstacles of great variety and complexity—the individual feels not only frustrated and inadequate, but baffled and bewildered in his effort to understand the relation between means and ends. He sees and feels the "real" world only in fragments, in bits and pieces. His ability to view the world directly is circumscribed by the tiny orbit of his own immediate environment. For the most part he must rely on others for information and knowledge about the forces at work to frustrate or to fulfill his dreams. It is on this knowledge that he must depend if he is to effect any rational accommodation of means and ends. Such knowledge is of greater importance in politics than in private life since politics has to do with

⁴ Sigmund Freud, *Beyond the Pleasure Principle*, New York, H. Liveright, 1950.

⁵ Karen Horney, *The Neurotic Personality of Our Time*, New York, W. W. Norton, 1937, p. 166.

⁶ Patrick Mullahy, *Oedipus: Myth and Complex*, New York, Evergreen, 1955, p. 67.

"public affairs" which by definition are more remote, having to do with "consequences which are projected in important ways beyond the persons or associations directly concerned with them." For knowledge concerning these public affairs, individuals are almost wholly dependent upon agencies of communication over which they have little or no control. These include not only the press in all its varied forms, from mammoth encyclopedias to flimsy throwaways, television, radio, and motion pictures, but also innumerable groups to which people belong. Few people are in any position to test the validity, reliability, or accuracy of what they read, see, or hear in these media. Hence political behavior in large measure is guided by gossip and rumor, myth and metaphor. The "face of the age" (in Rebecca West's phrase) which most people see is thus a compound of inadequate, inaccurate, incomplete, and—not infrequently—deliberately deceptive reports.

I mention all this because one source of the anxieties which generate political behavior is a product of this communications process.

For if people do not have the face of the age set clear before them, they begin to imagine it; and fantasy, if not disciplined by intellect and kept in faith with reality . . . dwells among the wishes and fears of childhood, and so sees life either as simply answering any prayer or as endlessly emitting nightmare monsters from a womb-like cave.⁷

In Freudian terms, when the ego function, i.e., the function of rationality, is "starved, atrophied, or tranquilized" in most members of the community, political behavior inevitably degenerates into an expression of mass anxieties, fears, and hates—which turn to some totemic father image for relief. Fascism in Italy, Hitlerism in Germany, and McCarthyism in the United States were notable examples of this phenomenon.

As the factual face of the world is dimmed, anxiety increases and the pressure of the fantasy-drives grows increasingly intense and successful. . . . The rage generated by the sense of being menaced by dangers that cannot be accurately understood, is partly turned against the weakly self, especially against elements of the self which bear any resemblance, genuine or fancied, to the caves from which nightmares teem.⁸

To understand politics, however, it is not enough to know that political behavior represents the "displacement of private affects upon public objects." Nor is it sufficient to know that these "private affects" arise from or are related to certain basic goals, i.e., physical and psychic values for which all men strive. Not all persons strive with equal in-

⁷ Rebecca West, *The Meaning of Treason*, New York, Viking, 1947, p. 56.

⁸ Harold D. Lasswell, "Propaganda and Mass Insecurity," *Psychiatry* 13: 283-299 (August 1950).

tensity for the same goals, nor do they always agree on the best ways and means for their realization. In some the drive for self-esteem and prestige or power will predominate over the drive for income and security, or love and affection. And even among those who share similar or even identical goals, differences may arise concerning the best means for achieving these shared values. It is these differences and the conflicts or tensions to which they give rise that account for the bewildering variety of human beliefs and behavior patterns. But conflicts over means and ends do not generate political behavior unless the outcome is thought to depend significantly upon factors in the external environment which can be deliberately controlled or manipulated. Only if it is widely assumed that certain canons of prescribed conduct inhibit or facilitate goal-achievement, or contribute to the relaxation of tension, will conflicts over ends and means result in political behavior. As Dewey would say, conflicts become political only when they have consequences which are projected in important ways beyond the persons or associations directly concerned with them and only when it is assumed that these consequences can be controlled by special agencies or by prescribed canons of conduct established for this purpose.

It is essential, therefore, if one is to understand politics, to inquire into the nature and source of value conflicts and the factors, both internal and external, which generate or significantly affect political as distinguished from other types of goal-directed activity. What we are concerned with is not the total environment of the individual or group and not even with its total psychological environment—but with those aspects of the psychological field that significantly affect political behavior. That is to say, we are concerned with what may be called a psycho-political field.

A useful concept for clarifying this problem of relevance is the so-called "funnel of causality" proposed in the manuscript of a Michigan Survey Research Center publication:

We wish to account for a single behavior which occurs at a fixed point in time. But it is behavior which seems to stem from a multitude of prior factors. It will be helpful to visualize the chain of events . . . as contained in a funnel of causality. Let us imagine the axis of the funnel as a time dimension. Events are conceived to follow each other in a converging sequence of causal chains moving from the mouth to the stem of the funnel. . . . Most of the complex events in the funnel occur as a result of multiple prior causes. Each such event is, in its turn, responsible for multiple effects as well, but our focus of interest narrows as we approach the dependent behavior. We progressively eliminate those effects which do not continue to have relevance for the political act.⁹

⁹ Michigan Survey Research Center, manuscript of "The American Voter," Ch. 2, as of January 1959.

We cannot here describe how the Michigan staff goes about factoring from any single cross section of this funnel the exogenous factors from relevant conditions. Theoretically, factors more remote in time and place from the particular behavior being studied are exogenous. This, however, is not always true and the "distinction between exogenous factors and relevant conditions is quite relative." What may be exogenous for legislative behavior in Sacramento in 1959 may be relevant for a general election in 1960.

The Survey Research Center:

... assumes that whatever effect distant events (i.e., being unemployed in 1933) may have on current political behavior the effect must be present and measurable in some form (i.e., suspicion of Republican domestic policy) just prior to the dependent event . . . Taking all the motivational variables as a system, the voter is seen as standing at the center of a field of forces as he deliberates over his vote decision. [Not all these forces will move in the same direction; there will be cross-pressures impelling the voter in different directions]. . . . By measuring these conflicting forces and calculating the resultant, it is possible to arrive at a prediction of the individual's vote.¹⁰

By rough rule of thumb one may distinguish among relevant factors or conditions those which are *personal* and those which are *external*. *Personal factors* would include all those physical or personality traits, ideas, habits, values, or attitudes which can be shown to affect political behavior. *External factors* would include all institutional or structural, demographic, economic, social, or cultural factors that are part of any individual or group's psycho-political field.¹¹

Both personal and external factors may be said to have a positive or negative "valence" (to use Kurt Lewin's term) when they result in an affirmative or negative response to any object, event, proposition, or person. Lasswell's "factors of indulgence" or "deprivation" thus become the key to the decision-making process whether at the polls; in legislative or judicial bodies; or in executive or administrative offices, boards, or commissions. Theoretically it should be possible to construct a "calculus of indulgence and deprivation" indicating the relative numbers and weight of positive or negative factors operating within the psycho-political field. With such a calculus one might conceivably forecast political decisions. Indeed, this is what popular forecasters do in a crude way when they undertake to predict elections on the basis of census data relating to wages and prices, employment and unemploy-

¹⁰ *Ibid.*

¹¹ The Michigan study defines *personal factors* as "those events or states within the funnel of which the individual is aware, although he need not conceptualize them as the investigator does;" *external conditions* "are those which warrant a place in the funnel because they are causally significant for later behavior, yet which currently lie beyond the awareness of the actor."

ment, and other indices of indulgence (prosperity) and deprivation (recession or depression).

Obviously, factors of indulgence and deprivation operate to affect decisions in every walk of life—in the family, the market place, the church, the trade union, the corporation and even, no doubt, in the process of courtship, marriage, and divorce. For the political scientists, however, this complex “funnel of causality” or this “calculus of indulgence or deprivation” comes to focus mainly on the participation of people in voting, or decision-making on “public” as distinguished from “private” affairs, and especially on the “public” affairs of the political state or community. The Survey Research Center in its study of the 1952 presidential election, *The Voter Decides*, suggested three main subdivisions of this broader universe of discourse—namely:

1. Issues.
2. Candidates.
3. Party affiliations.

These have now been extended to include “interest groupings” and general party policy and leadership. The basic research problem then becomes one of discovering which of the multitude of factors that “condition” human behavior in general can be said to operate significantly in determining the nature and extent of political participation as it relates to decisions on:

1. Issues.
2. Candidates.
3. Party affiliations.
4. Interest groupings.
5. General party policy.

I would not argue that these five categories are mutually exclusive or that, together, they can be said to comprise all that is significant in the study of politics. At best they help to bring the otherwise vague and diffuse field of political behavior into more manageable focus. Even within these limits the field of “research in politics” is a broad one and bristles not only with substantive problems but also with problems of scope and method. What, for example, is to be included under “issues” or “general party policy,” or “candidate”? Research in politics might well include objective analyses of public policies affecting agriculture, taxation, public finance, social welfare, foreign policy, and a host of other problems calling for political decisions. Studies of administrative, legislative, and judicial organization and procedure may be as important to the student of politics as studies of pressure groups and propaganda, since they, too, have an important bearing on political decisions affecting “issues” and “candidates.” Moreover, there are prob-

lems of method to which general reference has already been made. And these cut a wider swath than the choice between *panel techniques* and cross section samples. The skills of historians, constitutional lawyers, sociologists, psychologists, and philosophers, with a behavioral orientation, are as essential as those of the mathematical statistician, if we are to understand politics and politicians.

"RESEARCH IN POLITICS"—A MODEST PROPOSAL

"Research in Politics," therefore, includes interdisciplinary studies of contingent (or causal) relations between individuals or groups and relevant factors in their psycho-political fields. It would be presumptuous to attempt to outline in detail a comprehensive program of this kind. The following suggestions constitute only a modest proposal.

1. We need a good deal more information concerning the personal factors which affect political behavior. What, for example, is the relation, if any, between political participation, i.e., voting or nonvoting, and such factors as personal physique, sex, age, racial, ethnic or religious identity, education, structured attitudes toward human nature, authority, conformity, and conflict? What is the effect, if any, of such factors in determining not only who *will* vote but *how* they will vote, i.e., not only the probability of participation but the form and direction of such participation? *Ballots and the Democratic Class Struggle* (1943); *The People's Choice* (1948); *The Voter Decides* (1954); *The Middle Class Vote* (London, 1954); *Straight Fight* (London, 1954); *Class, Status and Power* (1953, a reader); *Voting* (1954); and *American Voting Behavior* (1959) are some present sources of information on these questions. The best over-all summary I've seen is the long chapter, "The Psychology of Voting," by Lipset, Lazarsfeld, Barton, and Linz, in the 1954 *Handbook of Social Psychology*. I have also found some of the sociological studies of social stratification and mobility helpful. Bernard Barber's volume on *Social Stratification* is a particularly useful review of this literature. Are there sex-linked and age-linked factors which help to explain patterns of political behavior? Is the lower rate of female voting due to basic attitudes of female inferiority? Are women more or less interested in certain issues than men? Is there a significant correlation between age and conservatism, or is this mainly a function of the memory of things past? If so, it is possible for older folk who suffered and remember the agonies of the great depression to be more liberal or even radical than their younger fellow voters. What political effects can we anticipate from a gradual aging of the population? Or of its "younging" when that occurs? We ought to apply this kind of analysis not only to the voter at the polls but to voting in legislative

bodies and executive agencies as well. What, for example, do we really know about the personal characteristics of politicians? Do we have any study comparable to Laski's analysis of the British Cabinet?

2. In what ways and to what extent is political behavior, i.e., voting behavior, contingent upon economic and social status? When the issues, candidates, party, or group preferences, with which the voter is confronted, engage interests associated with his economic and social status but also conflicting racial, religious, ethnic, or nationality interests, which—by and large—seem to prevail and under what circumstances? Under what circumstances will voters sacrifice economic and social status for other values?

3. Is there a significant relation between political participation and urban or rural residence? Size of community? Density or mobility of population? Remember Charles Titus's "law of political participation"? Is it in fact a pipe dream? Do such studies show any significant differences in the direction of voting behavior, i.e., are rural areas more or less Democratic than urban areas? Are such differences in fact rooted not in space-density-mobility factors but in economic, social, and ethnic factors?

4. How is individual voting behavior related to group membership, either directly or by reference? Patterns of political behavior transmitted through the family are said to be a major factor in determining the voting habits of wives, husbands, and children. To what extent is this true? What differences are there in the roles played in this respect by husbands/fathers and wives/mothers? What are the major mechanisms or processes by which parental political behavior patterns are transmitted, i.e., by imitation, precept, power, or influence? Does the size of the family and its composition, i.e., the number of boys and girls, effect its role as a major factor in determining political behavior? Is the family, as a transition belt for political behavior patterns, losing its importance? Are there significant differences in this respect between families of various racial, religious, or ethnic groups?

5. What, if any, is the political role of organized religion in the United States? We know from Lazarsfeld and others that there are significant differences in the voting behavior of Catholics, Protestants, and Jews. To what extent do these differences reflect organized efforts by the church or other religious groups to influence the political behavior of their members? This is a vast and largely unexplored field.

6. In what ways and to what extent do other organized groups influence the political behavior of their members? PAC and COPE are examples of such efforts by trade unions. What about chambers of commerce, corporations, trade associations, etc.? Andrew Hacker has

recently thrown some light on corporation programs for the political education of their executive and administrative personnel. We need more light on this problem.

7. We need more light, too, on political problems of internal group government. Seymour Lipset's essay on "The Political Powers in Trade Unions" and his impressive volume on *Union Democracy* point the way to better research in this field. Tannenbaum and Kahn's recent study, *Participation in Union Locals*, Grant McConnell's essay on "The Spirit of Private Government," and John Hutchinson's article on the new AFL-CIO constitution are other examples of the kind of research needed. We've been too easily satisfied with genuflections toward Michel's iron law of oligarchy. It's time for us to ask whether in fact it operates as Michel said it would in interest groups and other organizations including American political parties. And we ought not to expend all our energies on trade unions. Corporations, trade associations, and the major pressure groups that operate at all levels need an equally searching scrutiny. The work of Adolf Berle and Gardner Means on the *Modern Corporation* is of course indispensable to such inquiry. So, too, are some of the more specialized studies of corporation leadership, i.e., Warner and Abegglen, *Occupational Mobility in American Business* (1955); Newcomer, *The Big Business Executive* (1955); Mills, *The Power Elite* (1956).

8. There are a number of excellent general surveys of the role of pressure groups in American politics. Herring's *Group Representation Before Congress*, Donald Blaisdell's *Economic Power and Political Pressures*, and his *American Democracy Under Pressure* come readily to mind. But except for a very few studies like Schattschneider's *Politics, Pressures and the Tariff*, Bailey's *Congress Makes a Law*, and Garceau's study of the *American Medical Association*, there are few authoritative and detailed studies of the influence of particular special interest groups on voting behavior in legislative bodies. In a virtual wilderness of literature on pressure groups and pressure politics, there is little that goes beyond what is superficial, obvious, or dubious. An inquiry, for example, into the influences at work in shaping defense policy is long overdue. Even within the rigid limits of security regulations we ought to be able to learn a lot more than we now know. Such a study might even help us to recover some control of the Behemoth "defense" which now represents the major item in our federal budget and is probably a major—if not *the* major—inflationary force at work in our economy. Good studies of interest-group activity and influence at the state and local level are few and far between. There is a rich harvest to be reaped here for the research scholar. Lester Velie's article

on Artie Samish in *Collier's* a few years ago set in motion forces that helped to dethrone one of the most powerful state-lobby bosses in the U.S. Yet until recently, no scholar, to my knowledge, had even tried to review and analyze the Philbrick report which gave Velie his major "tips." Even the Kefauver Committee confined itself to one tiny segment of this problem. Every state, I am sure, and especially the major ones, bristle with opportunities. How are interest-group pressures on loyalties reflected in legislative voting patterns? Since Herman Beyle's *Attribute Cluster-Bloc: Identification and Analysis*, Stuart Rice's *Workers and Farmers in American Politics*, Donald Hecock's *Social and Economic Interests Revealed by Political and Geographical Cohesion in Congress*, we've developed some new and exciting methods for doing this kind of thing. George Belknap's use of Guttman's scaling techniques, for example, show what can be accomplished to reveal significant patterns of voting behavior in legislative bodies and David Truman's *Congressional Party* is a major contribution to this kind of analysis.

9. Not the least important of the activities of interest groups is the role they play in party politics. Yet little or no research has been done to reveal the scope and method of group influence on party organization and policy, nominations, party finance, campaigns, and elections. We get an occasional glimpse into these dark recesses when Congressional Committees fearfully and fitfully lift the lid on campaign contributions and expenditures. But not even this tells us very much about the role of interest groups in other phases of party politics.

10. Since Pendleton Herring's study of *Public Administration and the Public Interest*, we know that interest groups play an important role in the administrative process. Selznick's study of TVA, the Inter-University Case Program's studies of the *King's River Project*, the *Steel Seizure of 1952*, and a few other monographs of this kind have given us a close look at pressure politics and administrative policy. We need many more studies of this kind at all levels of government. We need to know, for example, just how and to what extent special-interest groups—presumably subject to governmental licensing and control—have in fact taken possession of the machinery by which these controls are administered. In California, for example, so far as I can discover, almost nothing is known about the working relations of some sixty state boards and commissions with the interests they are supposed to regulate. If present trends continue, we may find ourselves in a situation comparable to the one Henri Pirenne describes:

From the eleventh century onwards, the public authorities regulated town industry by dividing artisans into as many groups as there were distinct crafts

to supervise. Each of them (i.e., the crafts) had the right to reserve to its members the practice of the craft to which it devoted itself. There were thus essentially privileged bodies, as far removed as possible from industrial liberty. . . . Their monopoly was known in England by the name of *guild*, in Germany by the name of *Zunftzwang*.¹²

In the United States it is known as "regulation in the public interest" or, more properly perhaps, as "state enforcement of interest-group monopoly practices." Avery Leiserson's excellent study of *Administrative Regulation* offers not only new insights into techniques and tactics but also undertakes some analysis of the theoretical implications of group interests and administration. We should follow up on the leads he has given us.

11. I hope, in suggesting studies of this kind, I have not trespassed on the sensibilities of those who believe with Frank Goodnow that politics and administration are two different worlds. I happen to be one who believes that some distinction must be made between politics and administration, although neither Woodrow Wilson nor Goodnow in my judgment, defines these terms satisfactorily. One possible basis for distinction between politics and administration is the degree of ambiguity involved in the problems or propositions with which they deal. The greater the degree of ambiguity the more can it be described as "political." As Stimson Bullitt puts it in a charming essay:

When a politician gives his own opinions, he shuns exactitude. His words have rounded edges for he knows he may be misunderstood. Clarity is perilous unless transfigured by enchantment.¹³

Administration becomes possible only as ambiguity is reduced or eliminated. Since in the logical system which still governs us, ambiguity represents something akin to original sin—it follows that politics is nonlogical, or even nonrational, whereas administration admits, even requires, a high degree of logic or rationality. I prefer to believe that politics has a logic or a rationality of its own—a logic which admits of a high degree of ambiguity. Hence my earlier statement that politics is a more-or-less continuous exercise in the logic of ambiguity. Only at the lowest level of administration, where ambiguity has been finally exorcised and all discretion eliminated, are purely logical or rational methods—at least in the classical sense—strictly applicable. When this point is reached, politics vanishes and administration may be left to an army of robots armed with a battery of univacs.

12. The most significant work on group theory has been done by

¹² Henri Pirenne, *Economic and Social History of Medieval Europe*, New York, Harcourt, Brace, 1937.

¹³ Stimson Bullitt, *To Be a Politician*, Garden City, N.Y., Doubleday, 1959.

sociologists and social psychologists. The literature is extensive and growing in volume and I can claim no more than a nodding acquaintance with a small fraction of it. The work of George Homans, *The Human Group*; Robert Merton, *Social Theory and Social Structure*, especially his chapter on reference group behavior; Roethlisberger and Dickson, *Management and the Worker*; William Whyte, *Street Corner Society* and *The Organization Man*; Edward Shils, especially his essay on the "Primary Groups" in Lerner and Lasswell's *The Policy Sciences*; Harold Guetzkow, editor of *Groups, Leadership and Men*; Dorwin Cartwright, *Group Dynamics*; and Helen Jennings, *Leadership and Isolation*, among others, may seem remote and at best peripheral to "research in politics," but there are occasional ideas and insights that throw light on problems of political behavior. Studies in microsociology could conceivably help us to a better understanding of the committee process and its proper role in decision-making. Most research by political scientists on the committee system has been descriptive, hortatory, and superficial. A well-designed inquiry into the dynamics of committees by qualified specialists on small-group research might yield significant results for students of political behavior—whether their major interest happened to be the legislative or judicial process, pressure politics, or administration. But on this I speak as an outsider with little knowledge of the feasibility of such a project.

I have the impression, however, that Homan's and Festinger's "theory of interaction" and Wertheimer's "principles of perceptual organization," i.e., Proximity, Similarity, Common Fate, and Pregnance, would help us better to understand politically active interest groups. At least they can help to distinguish groups from non-groups. Can one say that the political effectiveness of any group depends upon (a) its size, (b) its "entitativity," and (c) its concentration in politically significant areas? Examination of certain racial and ethnic groups in New York and Los Angeles seem to suggest that this is true.¹⁴

13. The so-called Group Basis of Politics, I believe, needs a fresh review and probably a drastic overhauling. Except for David Truman's admirable gloss on Arthur Bentley's *Process of Government* in his own *Governmental Process* and Earl Latham's studies of *Basing Point Legislation* and *The Group Basis of Politics*, there has been no major attack on the many important theoretical problems involved. Now that economists—some at least—are proposing a theory of countervailing power as our road to freedom and welfare, there is a special need to re-examine the Group Basis of Politics, especially since it is offered as

¹⁴ See Donald Campbell, "Common Fate, Similarity, and Other Indices of the Status of Aggregates as Social Entities," *Behavioral Sciences* 3: 14-25 (January 1958).

a complete theory of political behavior. Just as Galbraith's system operates effectively only on a declining market, maybe the Group Basis of Politics operates effectively only in periods of widespread civic and scholarly apathy.

14. Not the least of the groups which play a major role in politics are political parties. In spite of a vast body of literature on parties, there are serious gaps in our knowledge of them. I know of no study of American party organization and governance comparable to Robert McKenzie's book on *British Political Parties*. McKenzie, you will recall, was concerned not merely with formal structure but with "the distribution of power within the Conservative and Labour parties." Not since Ostrogorski has there been any major effort to go behind the formal structural façade to discover the real centers of power within our major parties. A French scholar, Jean-Louis Seurin, has produced what is probably the best recent treatise in *La Structure Interne Des Partis Politiques Américains* (1953). Except for Ralph Goldman's dissertation on committee chairmen and Hugh Bone's recent book on *Party Committees and National Politics*, there have been no good operational studies even of the formal structure of our parties. Thanks to Hugh Bone, we now know something about party organization at the national level. State and local party organization, save in the most superficial sense, remains in stygian darkness. In V. O. Key's admirable book on *American State Politics*, he speaks of the "atrophy of party organization" but confines his attention almost exclusively to the alleged evil effects of the primary. I would suggest that some of the studies of power structures in "Yankee" and other cities or of power elites in Washington, New York, and Hollywood be extended to include a comprehensive survey and analysis of the structure of power within the major party systems. Is the traditional pattern of precinct, ward, and county organization outmoded in a world of mass media, interest-group dominance, million-dollar budgets and Madison Avenue or Montgomery Street leadership? If political bosses in the classic mold of Murphy, Penrose, Maschke and Flynn, Vare, Hague, Olvaney, and Roraback have vanished, who or what type of "leaders" have taken their places at the grass roots and at the summit? Does Michel's iron law of oligarchy operate at all in American party organization? If it does, where and in whom does power reside? I've seen no recent study comparable to William Mosher's survey of some 4,000 party officials in eighteen cities of New York. Is it because the traditional party leadership no longer has the significance it once had or was thought to have had? In California, at any rate, there is widespread interest in problems of party organization. In the 1959 Legislature, no less than 130 separate

measures dealing with parties and elections were introduced. Political scientists, as research scholars, can help legislators to find their way through this labyrinth.

15. To what extent has effective party leadership and control moved away from the formal legalized organization to nonofficial, more or less informal, organizations? The California Republican Assembly and the California Democratic Council are examples of nonofficial organizations which, if they have not displaced the official parties, have furnished a more dynamic and effective leadership. No really good study of these organizations in California or of similar ones elsewhere has come to my attention. Nor do I know of any serious or systematic study of the role played by theoretically nonpartisan interest and civic groups in strictly partisan politics. In most communities, business and professional organizations, chambers of commerce, and so forth, serve as adjuncts to the Republican party, and trade unions, by and large, play a similar role in Democratic party politics. If interaction based on proximity, similarity, and common fate, as Homans and Wertheimer suggest, is the basis of effective organization, it may well be that these nonofficial, "nonpartisan," organizations play a more significant role in party organization in the United States than do the official, formal party committees and conventions. Perhaps the greater effectiveness of the generally Republican business and professional groups helps to explain why Eugene Lee found that in California cities Republican "nonpartisan" candidates customarily win, even in overwhelmingly Democratic communities. "Of the twenty-six cities above 50,000 population in 1955, 80 per cent of the mayors and 68 per cent of the city councilmen were Republicans," i.e., "nonpartisan" Republicans. In nearly all these cities Democratic registrants outnumbered Republicans by something like 3 to 2.¹⁵ It is not surprising to find many Democratic leaders now calling for abolition of nonpartisan elections in California cities.

16. The nominating process has been described as the "jugular vein" of democratic government, and this is doubly so in one-party or quasi-one-party communities. From the Boston Caucus Club, meeting in the smoke-filled room of Tom Dawes in colonial America, to the party primaries and conventions of 1959, politically literate men have recognized that those who pick the candidates will "run the show." With the publication of the monumental study of *Presidential Nominating Politics in 1952*, by David-Moos-Goldman and associates, and the forthcoming Brookings volume of summary and analysis, *The Presidential Nominating Process*, we are beginning to know more about the most impor-

¹⁵ See Eugene C. Lee, "The Politics of Nonpartisanship in California City Elections" (paper presented at APSA meeting, September 1957), p. 15.

tant of all party nominating institutions—the national convention. Much remains to be told, especially about what goes on, not only in the convention itself and the smoke-filled rooms where delegates and others gather to deliberate but also in centers of political power outside this murky atmosphere. If the vice-president is to be restored again to his earlier status as heir-apparent, we'll want to do something about the altogether "chancey" process by which the second place on the presidential ticket is now assigned. I am hopeful that out of the Brookings study there may come some long-needed reforms in convention organization and procedure. But even without such gains, these studies, initiated by the American Political Science Association, are a monument to fruitful "research in politics." And if, as I assume, the national conventions continue to control the sole avenue of access to the Presidency, their importance will justify continuous attention by research scholars.

At the state and local levels there is no comparable study of the nominating convention. Ostrogorski's vivid—I almost wrote "livid"—account of state and local conventions has not been followed up by careful research on the evolution and present status of this important aspect of the nominating process in the United States. Books and articles on the direct primary are legion—but except for an occasional monograph, like R. N. Ballard's on *The Primary Convention System in Utah*, studies of the state and local convention system are few and far between, even though it survives in New York, Indiana, Connecticut, and Delaware for statewide offices and in other states for local and party officials. This is the more remarkable in view of the recent fairly widespread revolt against the direct primary and the growing interest in a restoration of the state and local nominating convention in some form. V. O. Key's brilliant book on *American State Politics* is a sustained attack on the primary. The indictment against the direct primary is familiar; (a) it is said to undermine party discipline and destroy party unity; (b) because vote participation is meagre, primary nominees represent only a minority of party voters and hence are often hand-picked by cliques and machine bosses; (c) it is said to open the way for factional, even "crackpot," candidates who could never get by a convention and who not only are unrepresentative of the party but who lower the level of statesmanship below what it would be if a delegate convention picked the party slate; (d) it is said to impose an intolerable burden on candidates by requiring them to conduct two campaigns; (e) it increases the burden on the ballot and asks the rank and file to make decisions for which they are poorly qualified; (f) and it affords no opportunity for deliberation in the formulation of party policy.

I happen to believe that there is overmuch blaming of the primary for "evils" that would not and could not be cured by a return to the delegate convention. But we need many more studies of this problem and especially of the pre-primary convention before we abandon the prevailing system. We ought not, in our ignorance, to throw the baby out with the bath water.

17. The early studies of presidential campaigns by Peel and Donnelley and Carroll Wooddy's excellent analysis of the Frank L. Smith campaign and the *Chicago Primary of 1926*, pointed the way to what might have become a valuable monographic series on political campaigns and elections. Unfortunately these failed to appear and—except for an occasional reference to campaign organization and techniques in The Erie County, Elmira, and the Michigan SRC studies—this important area of political research has been largely left to journalists and the hired men of candidates and professional public relations firms. Even if we include these, there are precious few global accounts of presidential crusades and even fewer comprehensive studies of particular state and local campaigns. Now and then someone like Jack Redding, *Inside the Democratic Party*, draws aside the veil that conceals the campaign machinery to show us how and in what directions the wheels go around. But Redding's book suffers from the inevitable bias of his own involvement in the Democratic campaign of 1948. Other studies such as the annual roundups of state by state campaigns published now and then in regional journals¹⁸ are little more than examples of academic journalese with almost no analysis of the theory and practice of political persuasion. We have done nothing in this country on campaigns and elections comparable to the excellent studies of British elections by David Butler and Herbert Nicholas.

Perhaps in our preoccupation with detailed problems of differential participation, motivation, and economic and social stratification of the electorate, we have lost sight of the more general problems of campaign planning, organization, strategy, and tactics. For this we are compelled to rely mainly on what can be gleaned from the *New York Times* and a few other journals which still print some news, reminiscences of professional politicians and campaign managers like Jim Farley, *Behind the Ballots*; Ed Flynn, *You're the Boss*; Ray Moley, *After Seven Years*; James Watson, *As I Knew Them*; and Clem Whitaker, an article or two, autobiographies, diaries, and biographies (authorized and otherwise) of Presidents, presidential candidates, and lesser lights, and general political histories, which usually do nothing of any significance with this

¹⁸ See Frank H. Jonas, "Western Politics and the 1958 Elections," *Western Political Quarterly* 12: 241-253 (March 1959).

problem. Samuel Adams, *Incredible Era*; Jonathan Daniels, *Man of Independence*; Frances Perkins, *The Roosevelt I Knew*; Burns, *Roosevelt: The Lion and the Fox*; Gosnell, *Champion Campaigner: F.D.R.* are but a few examples of where one goes to learn about campaigns. There are a few valuable studies of special problems or special aspects of political campaigning, i.e., Hugh Bone's "*Smear*" *Politics*; Ferguson and Schumucker, *Politics in the Press*; Kelley's *Professional Public Relations and Political Power*; Black's *Democratic Party Publicity in the 1940 Campaign*; Thomson's *Television and Presidential Politics*; Blumberg's *One Party Press?*; and an extensive list of articles on various aspects of campaign organization and propaganda. But from all this, few general principles to guide the harried campaign manager have emerged. Most of those who do research on these problems have been too close to the trees to see the forest.

18. Notwithstanding our preoccupation with special aspects of the over-all problem, our knowledge of campaign methods and technique is primitive at best. There are little or no scientific data, for example, in terms of which campaign managers can make a rational allocation of effort and expenditure among the various media or between the mass media and precinct organization, or between precinct organization and the mobilization of interest-group support. Herbert Simon, De Sola Poole, Angus Campbell and Warren Miller, Charles Seipmann, Gilbert Seldes, Bernard Berelson, Paul Lazarsfeld, C. R. Wright, and the Department of Marketing at Miami University, among others, have sought to examine the effectiveness of radio and television in political campaigns, with results meagre to null. Perhaps the networks, the advertising agencies, the survey and rating agencies (Hooper, Nielson, etc.), Motivation Research Associates, the Color Research Institute, the Advertising Research Foundation, and other members of Vance Packard's fraternal order of *Hidden Persuaders*—know the answers to these problems. If so, they have not as yet revealed them to the professional politicians or to the professional students of political behavior. Helpful compendia of research on attitudes and propaganda management are available—some of which are relevant to "research in politics." Wilbur Schram, Katz, Cartwright, Eldersveld and Lee, Berelson, and Janowitz have edited such collections. Most impressive is the recent monumental *Case Book on Psychological Warfare* by Dougherty and Janowitz. To really get at the problem will require the continued efforts of students of all of the behavioral sciences. Communication specialists, experts on public opinion, psychologists, sociologists, and social psychologists, will have to join forces with students of politics if we are to probe these depths.

I've always regretted that Bob Leigh was not able to finance his ambitious program of research on mass communications. He outlined four major problems for study, all of which still remain largely unexplored. These were (a) ownership and control, (b) content, (c) audience (actual and potential), and (d) effect. There is a good deal of information on a, b, and c but almost nothing of importance on d. Such an inquiry might throw great light on the use and abuse of mass media in politics. Perhaps the FCC or Congress or the Ford Foundation or Henry Luce will one day sponsor and support a research project of sufficient magnitude to surmount the superficial and self-serving rationalizations that now pass for scientific knowledge in this field.

Central to any effective campaign management is an adequate and reliable system of intelligence. Until recently, politicians and their handlers have looked upon public opinion polls with both fear and awe, taking comfort in them when the results confirmed their hopes and condemning them as "phony" when they didn't. There was a fiction widely entertained among politicians that although *they* personally took little or no stock in the polls and were uninfluenced by them, *their colleagues* were influenced and some even seemed to "hang on Gallup's coattails." Most of them, I suspect, were happy when the pollsters were discomfited in 1948. In recent years, however, survey research specialists are beginning to come into their own as chiefs of intelligence and even consultants on campaign policy and administration. Louis Harris has reported that by 1958 private surveys for candidates and political parties exceeded in dollar volume both newspaper-type polls (Gallup, Roper, etc.) and university-sponsored studies (\$1,000,000 on private surveys, \$300,000 for newspaper-published polls, and \$600,000 for university-sponsored surveys—*Prod*, March 1959). If this trend continues, opportunities for applied research assignments by academic students of political behavior will multiply—to the greater glory of "research in politics" and more rationally managed political campaigns.

If universities are to conduct significant research in problems of political organization and campaign management, at least two things must be done: (a) there must be a closer and more continuous liaison between academic research scholars and professional, practicing politicians, and not only those in official positions but those with the unofficial and informal organizations that directly or indirectly serve the official parties; and (b) we must establish, in every part of the country, centers for research and information on politics and political behavior. These centers should be repositories of political materials of all kinds: pamphlets; leaflets; posters; books; documents; tape recordings, including TV re-

cordings, etc., all bearing upon problems of political organization, nominations, campaigns and elections. The centers should be accessible not only to students of politics engaged in research but also to politicians engaged in the hazardous business of putting that research to work.

19. Campaign finance has been a perennial research problem for political scientists and a monumental headache for candidates and party officials. The early explorations of August Belmont, Theodore Roosevelt, William Beveridge, William Curtis, and other reformers, followed by James Pollock's study of *Party Campaign Funds* (1926), Sike's monograph on *State and Federal Corrupt Practices Legislation* (1928), and Louise Overacker's indispensable studies of *Campaign Funds*, developed the basic framework of public policy on this problem. Most of the data for this research have been produced by Congressional Committees on Privileges and Elections and a number of full-dress Congressional investigations of lobbying. The Kenyon, Steiwer, Caraway, Nye, and Gore committees, to cite a few, sought to reveal the scope and methods of campaign-financing and to recommend remedial legislation. Relying largely on these data, supplemented by some independent sampling by interview and questionnaire, political scientists have produced an impressive volume of research. There have even been a few efforts to study the problem on the state and local level.¹⁷

Aside from fact-finding, most of these investigations have had an essentially negative orientation. Congressmen and scholars alike have seen the problem mainly in terms of regulation and control rather than in terms of how best to finance party campaigns and elections. For the most part they have been content to rely on legal limitations on contributions both as to source and amount and the outlawry of certain types of expenditures.¹⁸ But mainly they have sought to compel public disclosure of contributions and expenditures on the theory that if you give light the people will find their own way. This is not the place to examine the validity or effectiveness of this policy. No one, I believe, would argue that it has been a howling success.

Without de-emphasizing the importance of disclosure, I should prefer to emphasize affirmative ways and means of party-financing. If, as is

¹⁷ See Hugh D. Price, "Campaign Finance in Massachusetts in 1952," *Public Policy* 6: 25-46 (1955); Elston E. Roady, "Florida's New Campaign Expense Law and the 1952 Democratic Gubernatorial Primaries," *American Political Science Review* 48(2): 465-476 (June 1954); and Leonard Rowe's forthcoming monograph, "Political Campaign Funds in California."

¹⁸ See Joseph Tanenhaus, "Organized Labor's Political Spending: The Law and Its Consequences," *Journal of Politics* 16(3): 441-471 (1954) and the note on "Statutory Regulation of Political Campaign Funds," *Harvard Law Review* 66(7): 1259-1273 (1953).

generally assumed (without much proof however),¹⁰ there is some relation between success at the polls and adequate funds at campaign headquarters, the problem of party finance may be a crucial one for the vigor and viability of our democratic society. If one party with abundant resources is able to command a disproportionate share (let alone a monopoly) of the mass media and the professional services that any effective campaigning now requires, while its poverty-stricken rivals must sulk silently on the side lines, we may in effect develop a one-party system. Unless rival parties and candidates have substantially equal access to the voter, it is difficult to see how our party system can present to the electorate meaningful alternatives of men and measures which we have always assumed to be one of their major functions. How to insure equitable access to the voters for rival candidates and parties strikes me as the major problem of party finance. It is to this problem that I would urge students of politics to direct their research. Some specific questions suggest themselves for systematic study: (a) What are the problems of policy and administration involved in making political contributions—within reasonable limits—deductable for income tax purposes? (b) On what terms could the franking privilege be extended to all candidates for public office? And what would such a reform cost? (c) Should the federal government and the state publish and distribute at public expense campaign bulletins containing material prepared by rival candidates or parties? And what has been Oregon's experience with a device similar to this? (d) Could the FCC be authorized, as a condition of licensing TV and radio stations, to require that a limited time be made available without cost to candidates of parties who qualify under the election code for a place on the ballot? (e) Short of this, should the FCC establish an upper limit on the time that can be purchased for political purposes on radio and TV stations? And what has been British experience on this? (f) Why should not every voter who seeks to enroll as a party member and to participate in the nomination of party candidates, be required, as a condition of such participation, to pay an annual or biennial enrollment fee, the proceeds to go to the party of his choice? Is such a proposal fatally defective as a poll tax? What are the major problems of policy and administration involved?

There is, finally, the proposal made successively by Theodore Roosevelt, Woodrow Wilson, and Senator Richard Neuberger, to have the federal government finance campaigns for national office. I have never seen the details of this proposal spelled out so as to answer the innumerable questions of public policy and administrative feasibility

¹⁰ See George Lundbert, "Campaign Expenditures and Election Results," *Social Forces* 6: 452-457 (March 1928).

which it involves. A bill to accomplish this on a state basis has been introduced in the California Legislature on several occasions, most recently in 1959; but with no research to answer its numerous critics and opponents (too numerous, in fact, to allow the bill to get out of committee), it has repeatedly failed.

Some recent unpublished studies by Herbert Alexander on Disclosure and on Tax Deductions and Tax Credits, Ellen Peters' article "Tax Incentives for Small Contributors," in *Louisiana Law Review*, April 1958, and John Van Dorn's monograph on *Big Money in Little Sums* (1956) indicate a commendable move away from a negative to an affirmative approach. And everyone, I assume, is looking forward to Alexander Heard's magnum opus on this problem. His preliminary essay on *Money and Politics* has only whetted our appetites.

20. Among the better-cultivated fields of political research are those which deal with election results and election trends, voting patterns and social stratification, and the general economic, demographic and social environment in which these occur. Professor Edgar Robinson's three volumes, *The Presidential Vote, 1896-1932*; *1936*; *They Voted for Roosevelt*; and *The Presidential Vote, 1932-1944*, and W. D. Burnham's *Presidential Ballots, 1836-1892* have been major resources for students working on these problems. Over a generation of effort by political scientists to persuade the U. S. Bureau of the Census to do something about a comprehensive record of elections in the U. S. has thus far proved all but futile. Research scholars are accordingly handicapped for lack of anything like complete returns on a national basis. Partially to fill this gap the Governmental Affairs Institute (with Richard Scammon as Director of Elections Research) is currently producing a handbook of contemporary American election statistics, called *America Votes*—of which two volumes have been issued. The most recent volume (1956-57) records both primary and general election results for each party on a state, county, and ward basis, plus "full tallies of each state's Senatorial vote since 1945, detailed maps of Congressional districts, state by state, and ward maps of all cities over 500,000." The Institute has under way a project for publishing similar data on elections in major foreign states. A comparable project on a state basis is the recently published report, *Kansas Votes*, containing the results of national returns in Kansas between 1859 and 1956, county by county, and district by district. Future volumes are planned to keep the record up to date and, says Ethan Allen, the "Governmental Research Center will also present, from time to time, analytical studies of electoral behavior in Kansas." These are pioneering efforts to provide

those seeking to do "research in politics" with basic data indispensable to such inquiries.

Students of elections have been handicapped not only by the lack of basic data of this kind but also by the fact that the indispensable census tracts showing economic and social composition of the population do not correspond to the political subdivisions in which votes are cast and recorded. It has been necessary therefore to supplement and check on census data by sample surveying for which few students of politics are either technically or financially equipped. In spite of these difficulties, much has been accomplished—so much, indeed, that one hesitates to cite particular examples lest the necessary omissions seem invidious. Stuart Rice's *Quantitative Methods in Politics*, James Pollock's *Voting Behavior, A Case Study*, Charles Titus', *Voting Behavior in the United States*, Arthur Holcombe's *Political Parties of Today*, and his *New Party Politics* are landmarks indicating a new (at the time) departure in the use of statistics, demographic and electoral, in political research.

Louis Bean's book on *How to Predict Elections* shows how one might apply simple statistical procedures to the study of elections without benefit of sample surveys. Less careful than these, perhaps, but more stimulating and exciting are Sam Lubbell's *Future of American Politics*, his less successful *Revolt of the Moderates*, and Louis Harris's frightening effort to discover whether there is a Republican Majority in America. Among the studies using survey sampling or panel techniques, *The People's Choice*, by Lazarsfeld, Berelson, and Gaudet, stands at the head of a lengthening list including the Michigan SRC volumes on the 1948 and 1952 presidential elections, and the Elmira report on the 1948 election by Lazarsfeld, Berelson, and associates. The methods employed in these and other American studies are now being extensively used abroad, where in England, Bonham's *The Middle Class Vote* and Milne and McKenzie's *Straight Fight* are examples. In France, Belgium, and Germany, too, similar methods are being applied to the study of parties and elections. There are still many rich veins to explore in this type of election survey and analysis.

In this areas as in others, however, we have had a tendency to deal with national elections—even when, as in Erie County and Elmira, our sample is drawn from a local community. State and local election studies, however, are growing in volume and significance. Gosnell's *Grass Roots in Politics* based on Wisconsin is a notable example, as indeed, are Leonard Reissmann's *New Orleans Votes*, C. W. Smith's *Electorate in the Alabama Community*, Ed Litchfield's *Voting Behavior in a Metropolitan Area* (Detroit), and Don Hecock's *Detroit Voters in Recent Elections*. And, of course, V. O. Key's book on *State*

Politics, although limited in scope, is a model of careful research. Broader in their orientation, if less exact in their methods, are Warren Moscow's illuminating *Politics in the Empire State*; Will Irwin's *Political Behavior in Colorado*; and Robert Pitchell's ambitious study of *California Politics*. The recently published *Bibliography on Western Politics* requires over 160 pages to list, by little more than title, research in the politics of eleven western states.

A bibliography of scientific behavioral research on regional or sectional politics, if there were one, would be less extensive. There is nothing comparable to V. O. Key's *Southern Politics*, for any other region in the country. Alfred DeGrazia's *Western Voter* is less a study of Western regional politics than a regional gloss on the Michigan Survey Center's study of the 1952 election, and Donnelley's *Rocky Mountain Politics* scarcely rises above the level of competent journalism. John Fenton's *Politics of the Border States* (i.e., Maryland, West Virginia, Kentucky, and Missouri) sets higher standards for itself and is particularly useful for its case studies of county politics. Rumor has it that V. O. Key will one day publish a volume like his *Southern Politics*, based on his study of politics in New England. Why shouldn't one of the foundations sponsor a number of regional studies of this kind? If we had a series on the West, the Middle West, New England, and the Middle Atlantic states, as companion volumes to *Southern Politics*, to supplement and give greater depth to national surveys, our knowledge of American political behavior could be vastly extended and enriched. Why shouldn't we combine projects of this kind with a quadrennial study of presidential elections on the order of David Butler's *The British General Election of 1951*, together with a careful sample survey like *The Voter Decides*?

21. I should also like to see more research that combines a tolerable acquaintance with contemporary behavioral techniques, with something of the social historian's perspective and the philosopher's concern for meaning and for values. Surely Beard's *Economic Interpretation of the Constitution* and his *Economic Origins of Jeffersonian Democracy* are not alien to what we now conceive of as "research in politics." Even if Forrest McDonald in *We the People* has demolished Beard's image of the Founding Fathers, it remains a monument not only to historical scholarship but also to political insight. The study by Arthur Schlesinger, Sr., of the colonial merchants in the American Revolution is a similar item that should be *must* reading for everyone interested in the Group Basis of Politics. Jameson's *Economic Consequences of the American Revolution*; W. J. Cash's *Mind of the South*; John Hick's *Populist Revolt*; the two volumes by Arthur Schlesinger, Jr., *The Age of Roosevelt*; and many others of similar import must also be included as "re-

search in politics." Why are there no really good histories of our major parties? In general we've done better by the minor parties, as Hicks, Barclay, Arnett, and others testify. One of our great needs is a new, comprehensive history of American politics. Wilfred Binkley's *American Political Parties* is almost alone in the field.

22. No program of "research in politics" can be defended if it neglects the influence of political institutions, customs, and conventions upon political behavior. To examine, or even to outline, the full scope and importance of the problem here is obviously impossible. Our preoccupation in recent years with such behavioral problems as motivation, stratification, group dynamics, etc., and the methods best adapted for their study has resulted in an often scornful neglect of the effects which institutional arrangements, tradition, custom, and convention can exert on behavior. Maurice Duverger may not be wholly right in attributing the basic pattern of party life, i.e., whether it be a one-party, two-party or multiple-party system, to variations in the electoral system. But to say that the single-member district or proportional representation, a federal versus a unitary system of government, a parliamentary system as opposed to one based on the separation of powers with a strong independent executive, have no influence or only minimal and negligible influence on political behavior, is, I believe, to be deaf, dumb, and blind. Moreover, we know that the overburdened ballot in the U. S. and ballot forms (party column versus office block), can and do have a profound effect upon voting behavior, party organization, and campaign strategy. Nominating systems, conventions, primaries (whether wide open, open, or closed) are certainly as much a part of the "psychological field" of parties, candidates, and voters as their economic and social status. The radical change in the pattern of California political behavior since 1952 has been due in no small measure to the mandatory Democratic-Republican ballot designation which first became effective in the 1954 primaries. And now with the final repeal of cross-filing by the 1959 Legislature, one can already sense, and even measure, a marked change in the political atmosphere both in Sacramento and in the State at large. Democrats gleefully and Republicans ruefully now talk not of "creeping socialism" but of "creeping partisanship," and some measure of party integrity and responsibility may once again be restored. Our next step may be to abolish nonpartisanship in local or at least in metropolitan politics in the not unreasonable hope that this may help to arouse the now apathetic urban voter to greater interest and participation.

The integral relation between political institutions and political behavior is, I believe, now recognized even by the most dedicated and zealous behavioralist. But the manner and measure of these influences

are by no means clear. Does gerrymandering, for example, affect the voting behavior of representatives who come from these secure havens of one-party dominance? Are they more or less loyal to the party that has done so kindly by them? Are they perhaps more independent due to their relative invincibility? Participation at the polls in districts without serious contests is generally lower than in districts where rival candidates battle it out in a reasonably fair and equal fight. Does the pattern of participation in gerrymandered districts illustrate this general rule? If so—and assuming active and widespread participation to be preferable to general apathy—should one test of a sound reapportionment measure be the degree to which it creates not “safe” but doubtful districts?

Is the disproportionate representation of rural as compared with urban population in Congress and in our state legislatures reflected in voting patterns of rural and urban legislators? In what way and to what degree do these differences appear? Clear evidence of an urban-rural conflict on major matters of policy might help to change the present representative system and the so-called “rule of the cow counties.” We may find in fact that the rural-urban conflict is largely hypothetical. Up to now we are mainly guessing.

What has happened to the many patronage studies that used to appear? So far as I know, there has been no reliable census or analysis of available partisan patronage in the Government of California. Everyone knows vaguely about the inheritance tax appraisers, juicy plums at the disposal of the State Controller, the patronage and prerequisites available to other state executive officers as well as legislators, especially those on important interim committees. But how extensive is all this and what, if anything, does it have to do with the party as a whole or with the capacity of various incumbent officers to build little party machines of their own? In general has the plural executive in our states had any measurable effect on party organization, internal discipline, and cohesion? Has it had a measurable effect on public policy?

Although one road to the White House is through the governor’s mansion, we know precious little about the office of governor and his position as a party leader. Shortly after his election as Governor of New Jersey, Woodrow Wilson told a conference of Democratic legislative leaders, “Gentlemen, I have been elected Governor of New Jersey, selected by the Convention of the Democratic Party, and I thereby have become the responsible leader of the Democratic Party in the state. . . . I am the only person . . . [who can] express approval or disapproval on behalf of all the people.” To what extent is this theory of the Governor’s role shared by present-day incumbents? If not, what is their image of the governor and his office?

I think that students of politics ought to take another look at the courts, not with a view to the derivation of mathematical systems for predicting judges' opinions, but as a critical part of the machinery by which important decisions concerning public policy are made. Judicial behavior, especially when it has to do with questions of competence and power, is simply a specialized form of political behavior. Whether or not Mr. Dooley was right, our state and federal courts, and the Supreme Court in particular, frequently find themselves at the center of political storms quite as violent as those that swirl about the White House. Judges, too, are men, whose behavior is conditioned by their own personal and social heritage and by the events that transpire in what Lewin would call their psychological field. We can understand, perhaps within limits, even predict, that behavior, if we remember that an important part of their "conditioning" is their notion of the role that judges should play in the American system of government. These ideas and attitudes as well as those on other matters that inform the judicial mind and conscience, can be discovered, as in the case of voters, governors, and legislators, by observing what they do and say. The study of judicial decisions in these terms, i.e., as a political process, is as much a proper subject of "research in politics" as is the decision-making process of voters at the polls and lawmakers in legislative halls.

For our courts and especially the Supreme Court not only discover the law, they also help to make it. Any doubt on this score should have been removed by the great decisions of the Warren Court on racial segregation in the schools. Moreover, in deciding as they do, from time to time, the legitimate (constitutional) scope of power of both the central government and the state and of Congress and the President, the Court makes decisions that are clearly political. It is quite necessary and proper therefore for the judicial process to be considered as a legitimate subject for "research in politics." Herman Pritchett's study of *The Roosevelt Court*, Robert Horn's *Groups and the Constitution*, Father O'Brien's article on "Justice Reed and Democratic Pluralism," and, indeed, a goodly portion of the literature on the Supreme Court, makes sense only in these terms. That the courts can even be used by pressure groups to do what they would be powerless to accomplish through the executive or legislative branch, is cogently argued by Walter F. Murphy in a recent article.²⁰

These are but a few examples to illustrate the importance of political and legal institutions, to the serious student of politics. I have said nothing about bicameralism, federalism, the committee system, the

²⁰ Walter F. Murphy, "The South Counterattacks: The Anti-NAACP Laws," *Western Political Quarterly* 12: 371-390 (June 1959).

seniority rule, the filibuster, the President as party leader, party government in Congress, and dozens of other institutional, legal, or quasi-legal arrangements that affect political behavior. They need much more careful and critical scrutiny by students with a behavioral orientation. They have been too long left to the lawyers and the political taxonomists.

23. I said at the beginning that it would be difficult to set limits to a paper on "research in politics." But limits of time and patience, if not of subject matter must be set. I conclude, therefore, first with an apology for the length of this paper. As Hervey Allen said of *Anthony Adverse*, "I simply did not have time to be brief." Finally, I apologize for including no mention of what I regard as three or four centrally important areas of "research in politics." These are: (a) The problem of Ethics in government. (b) The vast area of substantive research that must be done on urgent and persistent problems of public policy. (c) A re-examination of the common assumption that our major parties have failed to offer the American people meaningful alternatives among which they can choose. Is it true that the Republican and Democratic Parties are but a new and enlarged version of Tweedledee and Tweedledum? I venture to suggest that this canon of American political folklore, like Mark Twain's death, has been greatly exaggerated. And (d) contemporary behavioral science has its roots in psychological and philosophical principles that are essentially deterministic. With men like Watson and A. P. Weiss, even reason (as "mind stuff") was suspect. With our major emphasis now on "conditioning," and "funnels of causality," what, if any, place remains for the traditional concepts of rationality and freedom? If I were to give top priority to any particular research problem in American politics it would be to urge an all-out inquiry into ways and means for expanding the role of rationality in the decision-making process.

Perhaps if these problems are approached as problems involving and affecting human behavior and human freedom and welfare, they will seem less sterile and forbidding. Political scientists ought not to leave the most important problems of policy and organization to the economists and sociologists on the one hand or to the CED and the Democratic Advisory Committee on the other.

Comment

IN THE CONTEXT, presumably, of the question: "What are the agenda and non-agenda of university bureaus of government-related research?" it is a pleasure to be given the opportunity to comment upon a research prospectus as broadly and sympathetically conceived as Peter Odegard's. The following remarks are intended in no less tolerant a spirit, and I hope the questions raised will be viewed as an effort to clarify, to discriminate among purposes, and above all to promote communication and understanding rather than to wave the banners of methodological controversy or to engage in pejorative criticism.

1. The first two sections of Odegard's paper deal, respectively, with the orientation in political research that in the past fifty years has acquired the label "behavioral," and secondly, with the distinguishing category or criterion of human events that we call "political." I for one have no quarrel with Odegard's formulation of the behavioral orientation toward the state of political science:

The study of politics . . . has had to wait upon the development of a social science [I would say "sciences"] in which the systematic study of human attitudes and behavior is at least as important as the study of the formal structure of institutions and the history . . . of social and political thought. Indeed only when political institutions and political philosophy *are* related to political behavior, can we hope to understand politics as a dynamic force in any society. (P. 39, 40.)

Nor would I question Odegard's identification of the "political" category of human events as those originating in personal and group conflict—expressed in intra- and inter-personal tensions, interests, beliefs—over ends or means of achieving shared values when those conflicts:

. . . have consequences which are projected in important ways beyond the persons or associations directly concerned with them and when it is assumed that such consequences can be controlled by special agencies or by prescribed canons of conduct established for this purpose. (P. 52.)

I would add a third condition, namely, when the consequences involve significant shifts in the personnel, organization, or distribution of decision-making authority for the political community. The data of politics include not only the value and interest conflicts that condition the political process and those that are shaped and conditioned by it but also the process itself.

2. Odegard's survey of the logical and historical implications of the behavioral orientation reveals quite adequately that it aims at no mere nominalistic translation of old problems and concepts into modernistic psychological jargon. He underlines the behaviorist's interdisciplinary sensitivity and concern with the "psycho-physical field," the phenomena of motivation, perception, and learning, and at the same time his concern with the demographic and social as well as the legal and economic dimensions of governmental and political behavior. He emphasizes the behaviorist's receptivity to efforts to find better empirical, quantitative, and logically systematic grounds for the propositions and generalizations of political knowledge, and he points to the behaviorist's unflagging dedication to functional explanation of how values, institutions, and behavior (or any other potentially useful set of analytical categories and indices¹) interact to provide the generative and operative power in the political system. These emphases constitute the genuine earmarks of the behavioral orientation, and if they have not arrived at the stage that can legitimately be described as scientific in spirit and method, excellent reasons are to be found in the peculiar separation of philosophy and theory in the training of political scientists.²

3. In his two introductory sections, Odegard omits, except by implication, any demonstration of the connection between his orientation and the conceptual framework underlying the research proposal in his third section. The statement on page 54 indicates that his behavioral approach is wholly permissive and eclectic with respect to the type and range of studies appropriate for political research. This, it seems to me, leaves unstated several important problems that the university bureau must face, and if he was not inclined to "preoccupy himself with methodology," at least he ought to provide it with grounds for deciding what sorts of problems are theoretically more interesting and possible, as distinct from a list which asserts that all fields of politics are teeming with unanswered questions and areas of ignorance. He would have helped us a great deal if he had given explicit consideration to three problems: (a) the appropriate relation between scientific (explanatory) and policy (prescriptive or programmatic) research; (b) the preferable model, or priorities among models, as guide(s) for significant inquiry; and (c) the required level of analysis, i.e., the relative emphasis upon exploratory, descriptive, hypothesis-testing, and system-building studies.

The following three paragraphs are designed to amplify what I

¹ See G. C. Homans, "Activity," "Sentiments," "Interaction," and "Norms," *The Human Group*, New York, Harcourt, Brace, 1950.

² See R. A. Dahl, "Political Theory: Truth or Consequences," *World Politics* 11: 89-102 (October 1958); and "Research in Political Behavior," *American Political Science Review* 46: 1003ff (December 1952).

understand to be the behavioral position with respect to these points. I hope I do not do violence to Odegard's thought or injustice to his method of formulating the problems.

4. The behavioral or scientific method in human affairs does not require an absolute separation between science and human values, as expressed in the doctrine of ethical relativism,³ but neither are they the same; they must be discriminated and put in proper relationship. Rationality or the intelligence process can and does modify both values (ends) and conduct (means), but I doubt whether we know how to specify the conditions of appropriate balance in the abstract. The requirements of scientific proof for assertions of knowledge about human behavior in the political world are different from the assertions of belief sufficient for declaring public purposes and prescribing forms and canons of administrative action. Scientific knowledge may be harmful, useful, relevant, or irrelevant to policy-makers, but the type of questions asked and the standards or test conditions required seem to involve almost a different mode of discourse. The consequences of this distinction for managers and directors of university bureaus are that basic research projects probably ought to be conducted by different personnel and under different auspices than policy-oriented projects. This is particularly true of research on "how to put ideas or institutional changes across." At the same time, the distinction does not mean the two groups cannot communicate or learn from each other. There are two practical questions. The first, as to the problem of how much scientific as compared with applied policy research the university bureau can afford; the answer depends upon such factors as the control structure and political environment of the university, availability of funds and qualified personnel, the inter-personal and organizational relationships with the political and other social science departments, etc. The second, how can scientific research contribute to public policy, is partly a philosophical question and partly an administrative question, depending upon clarification of whether the policy problem is a dispute over ends (meaning or priority), the efficacy of alternative means, or a difficulty in communication. But between properly trained researchers and administrators, there should be no barrier to identifying the kinds of knowledge appropriate to the assumed ends of policy. However, scientists, administrators, and others differ as to the allocation of energy and funds for research and as to the uses to which knowledge shall be put.

³ See Roger E. Money-Kyrle, *Psychoanalysis and Politics*, New York, W. W. Norton, 1951, and Harold D. Lasswell, "The Impact of Psychoanalytic Thinking on the Social Sciences," in Leonard D. White, *The State of the Social Sciences*, Chicago, University of Chicago Press, 1956, pp. 84-115.

Query: From a rigorous viewpoint, is not all political research applied rather than scientific?

5. With respect to the question of priority among the various theoretical research models that have been produced by the behavioral sciences, there is a great danger of merely idle disputation which Odegard wisely avoided, and—as he is well aware—much, if not most, scientific knowledge was produced under the influence of erroneous theories. I am predisposed to agree with Herbert Simon that we need “a theory of cells, a theory of organisms, of species, a theory of groups, or organizations, of societies” (and presumably of societal evolution).⁴ Although we accept as a matter of faith the seventeenth-eighteenth century doctrine of natural rights, no one any longer seriously believes in the model of the abstract Leviathan State facing the equally abstract individual from which the doctrine sprang. I suggest, however, that it is important for researchers in politics and their administrative sponsors to understand whether their “bets” are being placed on the knowledge “payoff” of study designs based upon “general systems” theory,⁵ “culture-and-personality” theory,⁶ “status and role” theory,⁷ the theory of groups,⁸ or upon a theory of socio-political systems,⁹ if for no better reason than to be able to connect up with what other people are doing and to realize the scope and limitations of our concepts and data. The problem of making sure of our own inner sense of significance, and of not making overpretentious, underpretentious, or “phony” claims to knowledge, is of particular importance to political scientists, who

⁴ See Herbert A. Simon, “On the Uses and Limitation of Models,” in White, *op. cit.*, pp. 76–77.

⁵ See Talcott Parsons, “Voting and the Equilibrium of the American Political System,” in Eugene L. Burdick and E. A. Brodbeck, *American Voting Behavior*, Glencoe, Ill., Free Press, 1959, pp. 80–120.

⁶ See M. Brewster Smith, “Opinions, Personality, and Political Behavior,” *American Political Science Review* 52(1): 1–17 (March 1958) and M. Brewster Smith et al., *Opinions and Personality*, New York, Wiley, 1956.

⁷ See Siegfried F. Nadel, *The Theory of Social Structure*, Glencoe, Ill., Free Press, 1957; Bernard Barber, *Social Stratification*, New York, Harcourt, Brace, 1957; A. W. Kornhauser, “Public Opinion and Social Class,” *American Journal of Sociology* 55: 333–345 (1950); and H. W. Riecken and G. C. Homans, “Psychological Aspects of Social Structure” (Ch. 22) and Seymour M. Lipset et al., “The Psychology of Voting” (Ch. 30) in Gardner Lindzey (ed.), *Handbook of Social Psychology*, Cambridge, Mass., Addison-Wesley, 1954.

⁸ See Ralph M. Stogdill, *Individual Behavior and Group Achievement; A Theory*, New York, Oxford University Press, 1959; Neal Gross et al., *Explorations in Role Analysis*, New York, Wiley, 1958; Elihu Katz and Paul F. Lazarsfeld, *Personal Influence*, Glencoe, Ill., Free Press, 1955; David B. Truman, *The Governmental Process; Political Interests and Public Opinion*, New York, Knopf, 1951; and G. C. Homans, *The Human Group*, New York, Harcourt, Brace, 1950.

⁹ See Gabriel A. Almond, “Comparative Political Systems,” *Journal of Politics* 18: 391ff (1956) and D. E. Apter, “A Comparative Method for the Study of Politics,” *American Journal of Sociology* 64: 221ff (1958).

should be professionally conscious of their internalized cultural opposition between their logico-empirical models of politics and their democratic, authoritarian, liberal, or conservative values.¹⁰ From a scientific standpoint, however, the question is not whether we need one or a combination of models but whether by consciously employing one and borrowing from others, we can increase the generalizing and predictive power of our theories.

Query: Does the university bureau need to go beyond self-clarification, functional description, to prediction of consequences?

6. Closely related to the problem of clarifying the significant empirical models for political research is the matter of the criterion the university bureau shall establish for purposes of declaring its own commitments to a research program, and inferentially, discriminating among potential applicants for research support. Shall the criterion be stated in terms of *professional or institutional specialization*, e.g., the recognized subject-matter subdivisions of public affairs, political and governmental processes, or public policy? Or of *analytical level*, e.g., functional description, intervariate analysis, causal theory? Or of *personal sensitivity and competence to analyze the critical, common functions performed by the political system* in varying institutional settings, class structures, stages of economic development, historico-political cultures?¹¹ It is in the latter context that I suggest the university bureau may most fruitfully discriminate between its agenda and non-agenda, and while Odegard would probably agree, and admit that there are more and less important categories among his twenty-three project heads, I am not sure that his implicit premises would take us in the same direction. I shall take the liberty of referring to his suggestions under my own, not-very-original, topic headings.

Query: Is the university bureau in a position to undertake analytical, comparative studies of political processes?

7. Personality and Citizenship. The first inclusive research category concerns the opinion-forming processes in society whereby the person learns to perceive power and authority relations, and acquires his attitudes, values, and expectations toward the political system, leaders, groups, and issues.¹² The empirical focus is on the individual, and the development of interviewing and data-processing techniques has en-

¹⁰ See Charles E. Lindblom, "In Praise of Political Science," *World Politics* 9: 240ff (Jan. 1957); Anatol Rapoport, "Various Meanings of 'Theory,'" *American Political Science Review* 52(4): 972ff (Dec. 1958); and Franz L. Neumann, *The Democratic and Authoritarian State*, Glencoe, Ill., Free Press, 1957, Ch. 1.

¹¹ See Gabriel A. Almond, "A Comparative Study of Interest Groups and the Political Process," *American Political Science Review* 52(1): 271 (March 1958).

¹² See Robert E. Lane, *Political Life; Why People Get Involved in Politics*, Glencoe, Ill., Free Press, 1959.

abled us to make great forward strides in clarifying the influence of cultural, demographic, and group-membership factors upon the quality and direction of political participation. Such studies require mastery of intensive socio-psychological skills and involve an investment, whether for systematic sample surveys or intensive methods of clinical and laboratory testing, that tend to inhibit their utility as primary foci of interest for the university bureau of government research. Organizations like the University of Michigan Institute for Social Research, the National Opinion Research Center (Chicago), and the Columbia University Bureau of Applied Social Research have established a prior jurisdiction that merits cooperation, not competition. The government research bureau need not turn its back on the individual but should focus on the challenging problems of *aggregate electoral analysis*, where as Odegard points out there is a vital need for adequate basic election statistics, so that the demographic composition of the population can be systematically compared with the political divisions of the electorate. Deliberate programs should be undertaken to disclose local, state, and regional election patterns, and, over a period of time, historical trends supplementing the Governmental Affairs Institute's efforts on the national level. Such trend and pattern studies should of course be related to relevant economic and social indices.

8. Intra- and Inter-Group Politics. Most of Odegard's research suggestions, and—not unnaturally—what most political scientists would find most congenial, fall under the headings of interest-group politics, party politics, and the processes of opinion formation (mass communication), and leadership recruitment that occur in the interplay between these political groups, the several policy-making institutions of the formal political system, and the authoritative adjustment of interest conflicts with respect to the system's requirements of survival (equilibrium level), consent, timing, and continuity.¹³ There is no question but that university bureaus of government research can usefully assist in the prosecution of carefully designed, cumulative field studies of interest-group politics, aimed at disclosing (a) personal factors bearing upon group cohesion, morale, and decision-making; (b) the origins, attributes, and role-types of leaders produced by different kinds of voluntary groups or official agencies; (c) the distinctive institutional styles of leader-responsibility and accountability to the members in

¹³ See Oliver Garceau, "Interest Group Theory in Political Research," *The Annals* 139: 105-112 (Sept. 1958); S. H. Beer, "Group Representation in Britain and in the U. S.," *The Annals* 139: 131-140 (Sept. 1958); S. J. Eldersveld, "American Interest Groups," in H. W. Ehrmann, *Interest Groups on Four Continents*, Pittsburgh, University of Pittsburgh Press, 1958, pp. 173-196.

different groups;¹⁴ and (d) the functional purposes in group life served by the articulation of group activities, identifications, demands, and expectations.

9. Party Politics. While political parties may be viewed as a subspecies of political group, in developed political systems they acquire a distinctive position in both functional and symbolic terms. They may be analyzed with respect to the same variables suggested for the analysis of groups, or, in more traditional language of our "cognitive map," they certainly ought to be studied intensively and comparatively at state and local levels to throw light on the location and distribution of power in party structure, leadership recruitment from both inside and outside the formal party organization (the nominating, coöpting, and group infiltration processes), the effective processes of membership and followership, information, and communication. Studies of the planning, organization, and conduct of political campaigns, particularly those bearing on state and local levels of government on which the bureau makes organization and policy studies, deserve a high priority on the agenda of political research. These are naturally and easily related to inquiries into the control, use, and direction (content) of the mass media, whose control system constitutes an interest group elite of major political importance, and into the sources and objects of "political money."

10. The Political System: Structures, Institutions, Practices. When attention is focused upon the policy- and decision-making end of the political continuum, several approaches are available. The orthodox descriptive-analytical study in the pattern laid down by Herring, Schattschneider, Zeller, McKean, Bailey, Riggs no longer seems to be in vogue, but the conceptual pattern(s) by which they are being replaced is not clear. The case study approach followed by the Inter-University Case Program and the Eagleton Foundation deliberately avoids a *a priori* definition of "case types" or "situations"; the "decision-making" schema of R. C. Snyder and others¹⁵ still seems to be wavering between a general theory of organization and a residual, catch-all category capable of subsuming all types of policy-making behaviors and situations. Between these extremes of elaborate over-conceptualization and none at all, the prospects of fruitful research into the centuries-old processes of *representation* and *responsibility* are perennially chal-

¹⁴ See Seymour M. Lipset et al., *Union Democracy; the Internal Politics of the International Typographical Union*, Glencoe, Ill., Free Press, 1956, and Avery Leiserson, "Problems of Representation in the Government of Private Groups," *Journal of Politics* 11(3): 566-577 (August 1949)

¹⁵ See "A Decision-Making Approach to the Study of Political Phenomena," in Roland A. Young, *Approaches to the Study of Politics*, Evanston, Ill., Northwestern University Press, 1958, pp. ix-xiii and 3-38.

lenging and important. As Odegard says, we are completing the circle by analyzing the reciprocal influences between political institutions, customs, conventions, and behavior. Organization and role theory undoubtedly have something to contribute: the recruitment and circulation of elites, the concepts of *jurisdictional competence*, the facilitative processes of *communication* and *information*, and the variables impinging upon the conscious and unconscious motivations of *actor-decision-makers* in public office are promising guidelines for both systematic sampling and cumulatively designed study.

11. The Problem of the "Public Interest." Realistic investigation of norms of political, administrative, and judicial conduct are of the highest importance, but it is also of importance to distinguish such studies from the over-hasty urge to arrive at an "essentialist" definition of the public interest in general. Inquiries into standards of public conduct should not be mixed up with studies seeking to clarify in meaningfully descriptive ways how the public interest came to be formulated and striven after as an operational process in different areas of the public life.¹⁸

There are, of course, technical criteria of public interest that can be applied to research in policy-making in particular subject-matter areas; I refer to the concepts (elaborated by Homans, Truman, Garceau, Latham, Easton, and others) of viability, or equilibrium, among the politically significant groups in the polity, and to the more rational notions of *coherence*, *continuity*, and *timing*. Unless some such distinction of design and purpose are kept in mind, policy and decision-making studies are likely to deteriorate to "meticulous observation of the trivial," repetitious, descriptive monographs with a view of argumentative evaluation in terms of sin vs. virtue, or, at the most, to discriminating the effects of policy or institutions (e.g., legislative apportionment, executive patronage, party structure) upon the political fortunes of this desired or that disliked "group," or to verbal consistency with this or that "ideological" position. For an institution supported by public or philanthropic funds it is important to avoid the extremes of nonconsequential, noncontroversial projects and the self-defeating, if not suicidal, path of group or political partisanship. And so I conclude by restating Odegard's concluding point: The university bureau must formulate explicitly its criteria of public interest in terms of the kinds of intelligence (knowledge) appropriate to the alternative reality-choices for survival (adaptation) faced by the political community.

¹⁸ See G. A. Schubert, "The Public Interest in Administrative Decision-Making," *American Political Science Review* 51: 346 (June 1957) and Wayne A. R. Leys and Carmen M. Perry, *Philosophy and the Public Interest*, Chicago, Committee to Advance Original Work in Philosophy, 1959.

Comparative Studies

HISTORY OF GOVERNMENT-RELATED BUREAUS

The study of politics has, in the last decade or so, moved rather aggressively in the direction of becoming a *social science*. This is a new role for a field—it is certainly not now a discipline, in the sense of having a systematic, cumulative body of theory—that has grown out of the traditions of law, philosophy, and history. The rapid rise to importance of relevant scientific methods, largely pioneered in the social sciences by sociology, psychology, and economics, has, not surprisingly, left the field somewhat uncoordinated, strife-ridden, self-conscious, lacking in points of departure for theoretical development, and attached to traditions that are no longer appropriate. In particular, political science retains traditions that were borrowed from the humanities and are not wholly compatible with the scientific method. This is probably particularly true of the treasured practice of individual scholarship, which is both natural and fitting for the humanities but which has militated against certain practices, to be discussed below, that would encourage the use of scientific paraphernalia and might accelerate the development of a theoretical structure for a science of politics and government.

The changes taking place in those political science departments that prepare most of the future college and university teachers in the field have not taken place at a similar rate in the various bureaus of governmental research and related institutions, which have been manned wholly or largely by political scientists. There are probably many reasons for this. For one thing, although the bureaus have generally had the prestigious term “research” included somewhere in their titles, many of them were originally conceived of as elementary service institutions. As such, their research often consisted of nothing more than the gathering of data and the preparation of synopses. Even where emphasis was placed upon the collection of quantitative materials, few staff members (if any) had training in simple statistics, to say nothing of the more complex aspects of that discipline. The bureaus have probably often been viewed by administrative officials of universities as part of a public relations operation, particularly as they serve legislators. In this role, directors early found they could most benefit their agencies by producing materials pointing in familiar and expected directions. As such,

bureau research did not, unlike much basic research, challenge the assumptions of a major political myth, such as that of efficiency and economy. To the extent that bureaus have been lacking in a tradition of *analytical* research, the newer tools of political science have been of little value to them. They did not spring to the use of elaborate methodology. Indeed, despite the fact that many who worked in the bureaus were, in their own way, among the earliest empiricists in political science, behaviorism generally was developed outside these agencies—often, indeed, under the leadership of persons who were not political scientists.

There have been other reasons why bureaus have followed rather than led the parade toward an analytical, empirical political science. Their early sponsorship tended to limit their activities to the field of state and local government and many of them did not, therefore, flourish with each successive “vogue” field in political science. Instead of riding the crest of interest in public law in the thirties, international relations in the forties, public administration in the early fifties, and behavioral methodology since then, the bureaus became “typed” by other political scientists and even by their own staffs, and the type fitted the pattern of the twenties.

Other reasons may be pointed out, too. Modest budgets, which have been common, discouraged experimentation in new and apparently costly techniques. This in turn begat conventionality, which in turn left the bureaus in an unappealing position when the Era of Foundations began making its impact upon the academic world. The cultist tendencies among some of the early (and contemporary) behaviorists—the use of “vogue” terms and phrases when technical terms already existed or simple English-language terms were already available, pedantic citations of persons in the other social sciences and unfamiliar to conventional political scientists, the ponderous documentation of the obvious—created the impression both that the new techniques were only for ordained members of the priesthood and that their activities did not fall within the boundaries of political science. Furthermore, the detached scientific attitude of the behaviorists may have seemed on superficial examination to be in discord with the objectives of the kind of research that was the bread and butter of bureaus. It not only moved irreverently wherever its investigations led, but it was hostile to the hortatory types of publication. And while some behaviorists could see the day of a science of policy-making, which should benefit bureaus, they expected that it lay a long way in the future.

At the end of the fifties, bureaus are giving indications of interest in behavioral research techniques, in the study of all branches of

political science (not just the traditional subject-matter of "state and local government"), and in the development of theory. It seems to me that the Berkeley conference is held at a time of significant change in the role of the government-related bureaus in regard to the development of political science. University administrators are beginning to see bureaus of governmental research not simply as devices for cultivating legislatures but as possible devices both for securing additional research funds and as publication outlets useful in adding to the school's prestige.

The bureaus may be fortunate that most of them have not been in the thick of the fast-moving early development of behavioral political science. Unless they fail entirely to catch the train as it passes, they should be able to avoid having to experience some of the awkward, time-consuming and sometimes embarrassing events of the early years of the development of an empirical-analytical political science.

Bureaus and similar institutions should succeed, in the future, in finding ways to conduct research at several "levels" simultaneously. There will be no need for them to have a single purpose. They can serve legislatures in bill-drafting and in the gathering of gross data; they can discuss and summarize what is considered to be "best practice" in various areas of public administration; and they can aid in the experiments toward making political science a theoretical and hence a predictive science. There is no reason why "pure" research cannot be undertaken simultaneously with "applied" research within the same institution, even by the same individuals. Logically, the opposite assumption, that they can and should take place on an integrated basis, seems the correct one. Perhaps I am wrong in feeling an undercurrent of belief among my political science colleagues to the effect that the two are antithetical. To the extent that such a view exists, it probably stems from the legacy left us by the humanities. It does not accord with the evidence we have on the development of science and technology, which have been intertwined in organization and in the history of their progress throughout the industrial age. On the other hand, it seems to me that no bureau that is either a part of a college of liberal arts, or that is manned principally by doctoral graduates in political science, can justify a policy that omits the kind of basic research which seeks to explain the political process and to make political science a discipline. Given the current status of accumulated data and theory in the field, no agency that seeks to inform the general citizenry, or to serve in an even modest advisory capacity to legislators and charter commissioners, can be allowed to leave the fashioning of the building blocks to others. The low status of bureaus on some campuses—a status

that has damaged the standing of state and local government as a sub-field—is related to their failure to use political scientists as political scientists. One of the great needs in any service bureau is to allow a budget of time to permit every person to engage in some theoretical, non-project-related research.

GENERAL ASSUMPTIONS

In writing this article, I have made several assumptions. I will try to make the most important of these explicit. I assume:

1. That government-related bureaus must do academically respectable work if they are to exist within a University framework.
2. That these bureaus have, in recent years, slipped loose from the ideological moorings once provided by the efficiency and economy movement and that they have not found an equally secure anchorage.
3. That research of an empirical sort is needed to find a new, confident basis upon which advice in service projects can be based.
4. That this research need not be done by bureaus, but that it *can* be done by them or can be sponsored by them.
5. That bureaus are likely to remain firmly committed to academic traditions and standards if some of their personnel is engaged in both research and service work.

6. That comparative studies represent a possible approach to bureau research, though not necessarily the one deserving of highest priority.

I will not attempt here to offer a general conceptual scheme for analysis of the state and local government process, to provide an annotated bibliography, or to discuss methodology in detail. My goal will be the more modest one of outlining some of the areas where comparative research among bureaus of government and similar institutions might be both possible and desirable.

GOVERNMENTS

A comparative approach to the study of state and local government, it seems to me, should logically involve the following steps, generally in the order given:

1. The compilation and widespread distribution of existing literature so as to encourage its mutual utilization.
2. The development of a standard language.
3. The formulation of theories of the political process generally and of that at the state and local levels particularly.
4. The testing of hypotheses—though proof is generally impossible when theoretical propositions are tested against actual situations as we find them in the real world of social phenomena.

In addition, background preparation as well as the nature of the pressures that exist upon bureaus would seem to indicate special attention to two kinds of comparative studies:

1. The comparative study of policy-oriented problems.
2. The comparative study of regions.¹

The comparative study of governments has a threefold function:

1. To explain variables in light of analytical schemes and to develop a body of at least partially verified knowledge.
2. To appraise policy measures and identify problems and trends.
3. To reach a stage where prediction of trends or processes is possible.²

There is no reason to be very optimistic at this time concerning the eventual possibility of prediction concerning the real world. Furthermore, formal research often has no direct application to live situations, and at best it is likely to produce only flashes of insight rather than a blueprint for the phenomenal world. Perhaps we cannot have a theory of the total political process but we can have a theory of some of the parts of it. And this theory, if it survives empirical analysis, can in some cases—to take the least hopeful point of view—be applied to actual situations of the sort that confront bureaus from day to day.

The traditional approach to political science with its emphasis upon formal institutions described under static conditions—that is, without considering time sequence—clearly did not meet the conditions necessary to perform the above functions. It is easy today, of course, to make the traditional approach, dominant a generation ago, a straw man, easy to attack. Certainly information on formal structure as well as basic data generally is important. Some unkind critics have said, however, that bureaus have never done anything else. A review of the materials being produced today by bureaus of governmental research makes one wonder if the criticism is grossly exaggerated. True, little of the land-

¹ The procedures for comparative studies generally are outlined in Gideon Sjoberg, "Comparative Urban Sociology," in Robert K. Merton et al., *Sociology Today; Problems and Prospects*, New York, Basic Books, 1959.

These procedures are applied to state politics in Paul T. David, "Comparative State Politics and the Problem of Party Realignment," in Stephen K. Bailey et al., *Research Frontiers in Politics and Government*, Washington, The Brookings Institution, 1955, and they are applied to another area in W. J. Siffin, *Toward the Comparative Study of Public Administration*, Bloomington, Indiana University, Department of Government, 1957.

For bibliography and the status of international comparative government generally, see Roy C. Macridis, *The Study of Comparative Government*, Garden City, N.Y., Doubleday, 1955; Alfred de Grazia, *The Comparative Survey of European-American Political Behavior*, Mimeographed by the author, 1954; and Inter-University Summer Seminar on Comparative Politics, "Research in Comparative Politics," *American Political Science Review* 47: 641-675 (September 1953).

² See Macridis, *op. cit.*, p. 2.

mark material in postwar political science bears the mark of a government-related bureau, though some of it does.

Outside the bureau framework, a recent example of the inadequacy of large-scale traditional political science research is to be found in the American Commonwealth Series in which a rigid outline to be followed in describing "government and administration" in each state is substituted for a comparative conceptual framework. And the outline includes only those things that produce a legalistic, and to some extent historical, description of formal institutions. It does not include materials that could lead to a comparison of the process of policy-making in the states.

Compiling and Distributing Literature

The first of the steps in the comparative process would seem to be so elementary as to require no comment, and yet the fact is that those of us in the state and local field are really quite ignorant of the work that has been done in other states or the precise details of the governmental environment outside of the areas we have personally studied. If each of us were to make a special effort to accumulate the materials that do exist and to organize them within the regular university library so as to make them readily available not only to scholars but also to students, much of the parochialism of state and local government might disappear in a rather short time. Librarians have devised effective procedures for retrieving pamphlets and other materials that are difficult to file and hence easily lost.³ The major problem now is likely to center around the inadequate distribution of bureau-type publications to the libraries of research centers. This would seem to be easily remedied, but the important point is that it needs to be remedied.

Bureaus should also serve as major repositories of recent information about their states. They should have available or should automatically distribute recent election statistics, party organization information, and the like.

Developing a Standard Language

The development of a standard language has been discussed so often that it should require little space here. A science must have a standard set of terms, but the study of politics does not yet have one. The other social sciences have progressed more than we have in this direction.

In our early efforts, we seem to have followed many of the unfortu-

³ The librarian for our Institute for Community Development and Services at Michigan State University makes use of the "uniterm system" of filing, which requires neither IBM nor McBee equipment and, somewhat to our surprise, works well as a rapid means for retrieving filed materials.

nate mistakes of the sociologist. It is probably safe to say that a standard language will not follow from the use of obscure terms, chosen whimsically by individual scholars irrespective of nomenclature used by predecessors for the same phenomena. Political science has been hampered by the anarchic traditions of scholarship we have borrowed from the humanities.

The purpose of a standard language is to further precise communication, not to develop an intellectual elite with a monopoly of comprehension of terms. Yet, we find vague, inconsistent, and sometimes nonsensical use of such terms as "power structure," "ideology," "system," "role" and many others. Research bureaus should, by editing for style, be able to reduce the tendency toward uninhibited creation of new terms and eventually perhaps—this cannot be done quickly—achieve a standard basic vocabulary. This can—if properly done—contribute to readability and liveliness of style, rather than to obscurity. Such a proposal will probably produce rage responses from some persons in the field, and they will perhaps threaten not to work for bureaus that seek to impose such discipline; yet, if it is a discipline we seek, discipline we must have.

Formulating Theories

The formulation of theories and the testing of specific hypotheses is a basic part of the development of a science and, until very recently, we have had little of this in political science. Today, however, the state and local field is perhaps especially blessed because of the fact that empiricists have regarded the local arena, and to some extent state institutions, as manageable areas for research. Even so, empirical work has to a considerable extent been inductive or based only vaguely upon a conceptual frame. It is not easy to develop a general theory that is not empirically derived, but whose components can be empirically tested. The findings of research may be subject to so many interpretations that the practical result offers no net gain over untested theorizing; proof, lacking controlled experimental conditions, is not possible. Yet, efforts toward a comprehensive theory of politics are being made in a tentative fashion. The fact that the state and local field is somewhat less poverty-stricken than some other branches of political science so far as empirical data are concerned may also potentially mean that general theories of the political process might come forth earliest in the areas in which government-related bureaus are most active.⁴

⁴ On the development of theory generally, see David Easton, *The Political System, An Inquiry into the State of Political Science*, New York, Knopf, 1953; Robert M. MacIver, *The Web of Government*, New York, Macmillan, 1947; David B. Truman, *The Governmental Process; Political Interests and Public Opinion*, New York, Knopf, 1951;

The testing of hypotheses may be undertaken by replication, by simultaneous studies, or by moving from an agreed agenda. Each has its disadvantages. Replication is often impossible, as when the technique followed in studying the council in a city using at-large elections is to be applied to one using the ward system. An effort at near approximation, as might be attempted in the above example, is not, in a strict sense, replication. The technique also requires the acceptance by one scholar of a research design with which he will find himself in partial disagreement almost from the beginning, if he is true to tradition. Many research techniques, such as the use of unstructured depth interviewing, do not lend themselves to replication.

Studies that seek to use essentially the same research design in a number of studies—carried on simultaneously rather than *seriatim*—have been attempted in a few instances (e.g., the power structure cases now under way and sponsored by the Governmental Affairs Institute). Such studies of same phenomena (to the extent they exist) in several locations over the same period of time have the advantage of reducing the number of variables (as in the study of the 1952 nominating process in each state)⁵ but have the disadvantage of being less precise than replication in matters other than time sequence.

Doing comparative research by moving from an agreed agenda refers simply to the use of ideas found in such documents as those cited in this paper as a basis for a point of departure in lieu of a more precise comparative plan. In the absence of a theoretical foundation, a general agreement on the nature of the problems to be pursued and of an order of priorities should be preferable to random activity. Many typological agenda already exist for the introduction of interrelated hypotheses;⁶ we can expect them to appear in greater numbers in the years ahead.

Practical Tests of Hypotheses

Comparative studies by government-related bureaus had their first beginnings in activities related to policy-oriented problems. Early efforts of this kind tended to be grounded on the values of the efficiency

Bailey et al., *op. cit.*; Charles S. Hyneman, *The Study of Politics*, Urbana, University of Illinois Press, 1959; and Roland A. Young, *Approaches to the Study of Politics*, Evanston, Ill., Northwestern University Press, 1958.

⁵ See Paul T. David et al., *Presidential Nominating Politics in 1952*, Baltimore, Johns Hopkins Press, 1954.

⁶ See V. O. Key, Jr., *American State Politics; An Introduction*, New York, Knopf, 1956; William J. Keefe, "The Comparative Study of the Role of Political Parties in State Legislatures," *Western Political Quarterly* 9: 726-742 (September 1956) and citations; and my article "A Typology for Nonpartisan Elections," *Western Political Quarterly* 12: 449-458 (June 1959).

and economy movement and, given the answers by ideology rather than by empirical derivation, the results tended to be hortatory in character. They sought to explain the virtues of the executive budget, the unicameral legislature, the council-manager plan, the abolition of townships, and the like.⁷ These studies made no systematic comparative effort, but they did seek to use materials from one state or community in discussing another, and they did present issues of public policy in terms of "problems" that were "concrete manifestations of conflict or instability," the study of which was "related to the need of adopting policies that do away with the cause of instability or conflict."⁸ The studies did not generally, however, place the problem in its total political setting as related to other problems or issues and the *a priori* method militated against empirical inquiry. Furthermore, the static concept of movement toward "the solution" discouraged the development of an evolutionary theory of change, which would recognize that public policy is constantly in flux and that the diminution of conflict is a part of the eternal process of making social institutions functional under changing conditions. The policy-oriented-problems approach is, however, a logical part of the comparative method and is one, furthermore, that harmonizes particularly well with the kinds of demands felt by the bureau directors.

The problem approach must, if it is to be of more than transitory value, be purposive in character; it must seek to give intellectually honest explanations of political phenomena and to relate them to the development of a broad conceptual scheme of politics. Writings on the unicameral legislature in Nebraska, for example, rarely discuss its operation in light of the essentially one-party, agrarian, socially homogeneous character of the state. These and other factors would have to be considered in any genuine evaluation of the plan if it were to be evaluated for use in, for example, California, New York, or Louisiana. Knowing that the township has, in fact, been abolished in Iowa tells us nothing about the feasibility or desirability of doing away with it in the virile form it has in Michigan. The fact that the London metropolitan police force operates over most of the metropolitan area and might be shown by some empirical means, not yet devised, to be doing a good job does not mean that the residents of suburban Zilchville, where there has not been a murder or a headline-grabbing rape in over a decade, are wrong in refusing, by simple analogy, to accept a similar arrangement. The fact that the annexation of the urbanized fringe of city A led to

⁷ Dozens of citations to this kind of material may be found in W. Brooke Graves, *American State Government*, Boston, D. C. Heath, var. eds.; Clyde F. Snider, *Local Government in Rural America*, New York, Appleton-Century Crofts, 1957.

⁸ Macridis, *op. cit.*, p. 29.

greater efficiency in the utilization of labor and equipment may mean little to the residents of the fringe of city B to whom a dozen psychic and nonmonetary factors may seem more important. In all these examples and in dozens of other issues brought to the attention of bureaus, the problems approach has real advantage of flexibility and adaptability and it may lead to purposive research even prior to the development of a theoretical framework. Its applicability "to modest levels of empirical research at manageable levels of abstraction is its greatest merit."⁹

Policy-Oriented Problems

Another essentially inductive approach to comparative studies is to be found in the projects, not necessarily conducted by research bureaus, that have inquired into regional politics. These represent the state and local equivalent of the "area studies" branch of international comparative government. They implicitly or explicitly assume that there are cultural or economic factors that justify a regional study, though in some cases perhaps nothing more profound may be involved than the assumption that a few states are enough to tackle at any one time. Sociologists have attempted to do comparative community studies, but there is almost nothing in the political science literature of this type,¹⁰ except for the problems-oriented studies mentioned above.

Perhaps the earliest major effort to do an extensive regional study of politics was reported upon in T. C. Donnelly's *Rocky Mountain Politics*, published in 1940. This was followed a few years later by H. F. Gosnell's *Grass Roots Politics*, but elaborate studies based upon systematic interviewing and the statistical method did not appear until after World War II. V. O. Key, Jr., and Alexander Heard produced a classic study on the South in *Southern Politics*. It was based upon the idea that the region had certain cultural, ideological, economic, and political (e.g., the one-party system) characteristics in common, but that their relationship to the process of government had not been tested. Indeed, it was suspected that the conventional knowledge that held that the region had a homogeneous political pattern might be erroneous. The study,

⁹ See Macridis, *op. cit.*, pp. 32-33, for a cogent evaluation of this type of comparative approach.

¹⁰ See Sjoberg, *op. cit.*, and Hyneman, *op. cit.*, pp. 90-92.

Oliver P. Williams and I are conducting a comparative study of the political processes in four middle-sized, council-manager cities in Michigan. The first reports of our findings are to be found in my article, "Leadership and Decision-Making in Manager Cities: A Study of Three Communities," *Public Administration Review* 18: 208-213 (Summer 1958) and in my paper, "The Role of Councilmen in Community Policy-Making," delivered at the September 1959 meeting of the American Political Science Association.

though widely applauded, has not been attempted on a similar scale elsewhere, probably because of the time and skills that would be required and of the perversity of foundations in refusing to sponsor the replication of studies they have sponsored or—perish the thought—that have been sponsored by other foundations. There have, however, been more modest efforts at regional studies, each of them received with almost the same eagerness that a man lost on the desert accepts water. John H. Fenton in *Politics in the Border States* and Duane Lockard in *New England State Politics* have made especially important contributions. Fenton, using a modest version of the Key and Heard technique, sought to explain the pattern of politics in the border states on the basis of historical-ideological factors. Lockard sought “to make a comparative analysis concentrating on the variations between” the one- and two-party states in the region, and he paid particular attention to the role of ethnic subcultures. His study was essentially inductive, although he presents an imprecise hypothesis that “two-party politics is in some sense more democratic, more responsible, or more rational than one-party politics.”¹¹ Key has sought to encourage systematic empirical research in a book which offers an agenda that could involve all the bureaus in the nation for full-time work in comparative state politics, for the next couple of decades.¹²

The empirical comparative method exists, then, both as a concept and as a technique. The problem is to make use of it. My suggestions, which follow are based on the idea that government-related bureaus can and should contribute toward theories of the political process through the comparative method and that enough research has been done to make possible an agenda for correlated activity in the future.

Regions

We still play, almost without variation, on the theme presented by Harold Gosnell a generation ago:

The quantitative techniques promise to give some very fruitful results if significant hypotheses and relevant indexes can be devised.¹³

¹¹ See T. C. Donnelly, *Rocky Mountain Politics*, Albuquerque, The University of New Mexico Press, 1940; H. F. Gosnell, *Grass Roots Politics*, Washington, American Council on Public Affairs, 1942; V. O. Key, Jr., and Alexander Heard, *Southern Politics*, New York, Knopf, 1949; John H. Fenton, *Politics in the Border States*, New Orleans, Hauser Press, 1957; and Duane Lockard, *New England State Politics*, Princeton, Princeton University Press, 1959.

¹² See V. O. Key, Jr., *op. cit.*, and his *Politics, Parties and Pressure Groups*, New York, Crowell, 4th Ed., 1958. See also The American Assembly, *The Forty-Eight States*, 1955, which contains many ideas from which hypotheses for regional studies might be constructed, and Leon D. Epstein, *Politics in Wisconsin*, Madison, University of Wisconsin Press, 1958, which makes use of some of Key's materials; although his study is not comparative, it contributes to a theory of comparative state politics.

¹³ Gosnell, *op. cit.*, p. 146.

The quarrel is often with the concept of significance. It is so much easier to give the appearance of demolishing a hypothesis than it is to test and retest one, that not a few political scientists have virtually made a career of the former. Some of the quantitative techniques—for example, statistical analysis of gross voting data, and sample surveys—easily lend themselves to inductive usage and the avoidance of hypotheses. Others—for example, game theory—have tended to be hostages of those more interested in polishing the technique than in finding out something concerning the political process; to make the use of the method an end in itself is sometimes a temptation. In the remainder of this paper, I shall attempt to suggest, without much attention to methodology, some comparative studies that government-related bureaus might undertake, studies that would contribute to a body of knowledge about the political process and might also be appropriate to meet the expectations of support groups for these agencies.

In a sense, all research is comparative, of course. It compares findings in one case against a previous test, or against another case in which conditions were similar but not identical, or against a standard or model. Here, the term refers to efforts that will generally involve the cooperative efforts of two or more research bureaus seeking to learn how certain phenomena related to the governmental process appear in one state or community in relation to another.

Comparative studies, to do very much comparing, must seek to minimize the number of variables between studies, as was indicated above. Having in mind replication, simultaneous studies, or the use of an agreed agenda or research design, we need a set of “significant” hypotheses. An effort to provide a few will be made here.

The traditional inquiries into political institutions, along with current investigations of decision-making and “power structure,” seem to me to provide relevant contributions to an over-all theoretical framework, although they are each in themselves inadequate. David Truman’s *Governmental Process* is far more suitable but carries with it to some extent the self-imposed limitation of A. F. Bentley’s bias against consideration of motivational factors. If we are to move eventually toward predictability in the social sciences, it seems to me that certain factors (which cannot be considered here) will force a consideration of motivation. Motivation, in group context—the study of it in the individual alone is probably not adequate for our purposes—leads to a study of ideology. Yet, ideology by itself is also inadequate as a basis for comprehensive theory, for it does not explain the process whereby ideas and values are translated into public policy. A general theory of the political process, it seems to me, should combine the essential ideas

of Truman with those of MacIver, even though each of those gentlemen might not accept the idea of a possible synthesis.¹⁴ The concept of "influence" as developed by Lasswell can be worked into a total theoretical framework as a particular phase of intergroup action, it seems to me, for I see both power and influence as special manifestations of intergroup activity.

It seems to me that most of the elements of a political system can be analyzed within a framework that assumes that government is a part of the control mechanism within a social system.¹⁵ As a device for social control, it seeks to achieve consensus and to alleviate conflict through compromise. This becomes the case as soon as social groups grow complex enough to require government and is true in democracy as well as in other political systems. Furthermore, since government is only one means of social control, the assignment of the comprising function to governmental institutions varies in scope from one social system to another. Public policy, under this analysis, becomes the product of the influences of effective groups in society whether organized or not and whether within government or not. The answer to "who gets what, when, how," then centers around the determination of the conditions which produce influence (including power), whether those conditions involve personality, group organization and administration, ideological motivation and conflict, or social-psychological or legal factors relative to legitimacy. Studies investigating these questions cannot be undertaken easily without the help of persons trained in techniques beyond those used in traditional political science. At Michigan State University, both the Bureau of Social and Political Research and the Institute for Community Development and Services have persons from fields outside political science on their staffs, and I presume that this practice is in effect or is being considered elsewhere. Certainly working agreements with persons in various cognate fields are necessary to developing of an adequate understanding of some phases of the political process.

Can we inquire into the nature of consensus, conflict, and compromise? Or is this largely beyond our technical competence? It seems to me to be central to an understanding of the political process as I have sketched it above and as I believe trends in political science interests are leading.¹⁶ Perhaps some basic inquiry in the form of simultaneous studies might enable us to gain insight into social-conflict-resolution

¹⁴ See Truman, *op. cit.*; Arthur Fisher Bentley, *The Process of Government*, Evanston, Ill., Principia Press, 1908; and MacIver, *op. cit.*

¹⁵ See Talcott Parsons, "Some Highlights of the General Theory of Action," in Roland A. Young, *op. cit.*, and his citations.

¹⁶ See Lewis Coser, *The Functions of Social Conflict*, Glencoe, Ill., Free Press, 1956, and Ralph M. Goldman, "Conflict, Co-operation and Choice," in the papers written for the United States Air Force Behavioral Science Conference, University of New Mexico,

theory as a means for getting a better understanding of the role of political parties, interest groups, and administrative behavior. Certainly government-related bureaus need to have a better understanding than they now have of the factors that lead both to conflict and to the compromise of conflict if predictability is their ultimate goal or if they seek to inform public officials as to the best means of achieving compromise or of the nature of effective devices of manipulation.

Explanations of the relationships between groups and their leadership and between the leaderships of competing or cooperating groups are not developed as yet. Again, this seems to be essential if we are to understand why groups behave as they do. The measurement, or even the description, of ideological factors that influence policy decisions is not much understood either. Unless our bureau staffs understand the nature of ideology and do not let it interfere with their analytical efforts, little headway can be made toward a science of politics. It was less than a generation ago that some bureaus accepted uncritically the values of the efficiency and economy movement and were so taken with it that they acted as public relations or legitimation agencies for a point of view. This represented a useful social function, to be sure, but did not serve to make a contribution to science.

Certainly bureaus should be able to conduct some comparative studies into ideological factors.¹⁷ In what way do variations in cultural-ideological factors affect public policies? Why do the standards of state highways in use differ so greatly between two industrial states such as Michigan and Ohio? If ideological factors are involved in some way, a bureau asked by a legislative committee to explain such a difference should be prepared to explain in appropriate terms rather than to say "the gasoline tax has historically been higher in Michigan," though this is, to be sure, an explanation of sorts. And it would suggest a solution—raising the gasoline tax—but would not tell much about why the difference exists in the first place, or whether the proposed solution would be politically feasible. Why are state grants-in-aid to local governments rare in New England and common elsewhere? Are ideological factors involved? If so, can we measure them?¹⁸

Summer 1958. See also the interdisciplinary *Journal of Conflict Resolution*, which deals generally with the subject.

¹⁷ See Robin M. Williams, Jr., *American Society; A Sociological Interpretation*, New York, Knopf, 1951, for suggestive material for analytical schemes. See also my textbook, *State and Local Governments: A Study in the Political Process*, New York, McGraw-Hill, 1960, in which I have sketched out an elementary analysis for American domestic ideologies.

¹⁸ See Fenton, *op. cit.* He seeks to explain the political system of the border states partially in ideological terms.

Many historians, sociologists, and political scientists have sought to use ideology in

It is easy to say that governmental structure is a function of the culture. But just what does this mean? Why is the council-manager plan suspected in some Illinois communities of being a subversive plot but is "naturally" the plan to be adopted when yet another California municipality is created? Why has rapidly urbanizing, suburbanizing Indiana never permitted the use of the plan? What is involved in determining state and local tax structures other than the unwillingness of individuals and organizations to pay taxes? To what extent is the tax structure determined by ideological factors? What kinds and levels of taxes are permitted by what kinds of ideologies?

POLITICAL PARTIES

Beyond ideology, the structure of the policy-making process at the state and local levels would lend itself to many types of comparative analysis. Party organization and machinery can be compared from one state to another with the view in mind of testing the effect of various types of organization.¹⁹ Before legislatures and communities can be advised of the relationship between structure and political patterns, we shall probably have to learn something about handling more than one variable at a time.²⁰

By comparisons, we can learn more about the social sanctions that relate to the politicians' role, that is, to the rules of the political game. There has been no systematic gathering of the rules by which the game is played; the conditions under which they differ by state or region; the effects of these variations; and the causal factors, which may give clues as to how the rules change or can be changed.

Little is now known of political-party differences by state and community or of party organization patterns. This limitation may be serious enough if we are to gain an understanding of the political process. But even less is known of interest-group activities and patterns. A hand-

analysis, of course. Often they do so in connection with the subcultures of ethnic groups, as Lawrence H. Fuchs, *The Political Behavior of American Jews*, Glencoe, Ill., Free Press, 1957.

However, they may do so on a culture-of-the-region basis, as W. Lloyd Warner and Paul S. Lunt, *The Social Life of a Modern Community*, New Haven, Yale University Press, 1941, or John Dollard, *Caste and Class in a Southern Town*, Garden City, N.Y., Doubleday, 3rd. Ed., 1957.

Studies often concentrate on voting patterns and are not oriented toward the question of the influence of ideology of groups upon public policy relative to specific functions of government, including the vital question of attitudes toward service levels.

¹⁹ See V. O. Key, Jr., *American State Politics; An Introduction*, New York, Knopf, 1956; Paul T. David, "Comparative State Politics and the Problem of Party Realignment," Bailey et al., *op. cit.*; and elsewhere, in which groundwork on this is suggested.

²⁰ See Paul F. Lazarsfeld, "Problems in Methodology," in Merton et al., *op. cit.*

ful of studies have been conducted on the state level but almost none at the local.²¹

An effort was made in the report of the American Political Science Association, Committee on American Legislatures, to classify states as to the relative importance of interest groups,²² but no more rigorous analysis has been attempted, although simultaneous studies from an agreed research design might be possible for this purpose. Here may be the appropriate place to mention one possible approach to the always delicate question of how far one may go in analysis beyond the conventional knowledge stage when dealing with those who control the purse for most government-related bureaus. I would suggest that one method—possibly involving poltroonery in a sense, but pragmatic and of the kind of horse-trading legislators understand—would be for the bureaus in two or more states to make trades, each doing a state other than its own. This type of protective device, though rather transparent, should be effective.

Tied to the study of interest groups is the study of elections in communities where there is a nonpartisan ballot. This form of election is extremely widespread today, and my own investigations have indicated that it produces both a different political style and a different balance of forces from those of partisan elections. What are the implications of this for the total pattern of politics? What does it mean when we consider the fact that many communities seem to have neither political-party activity nor effective interest-group organization for local public policy-making? How is public policy made under such circumstances? What are the implications for representative government?²³ These kinds of questions can be tested comparatively by bureaus within the limits of modest budgets.

²¹ The studies by Belle Zeller and Roland N. McKean are well known; so is the article by Oliver Garceau and Corinne Silverman, "A Pressure Group and the Pressured," *American Political Science Review* 48(3): 672-691 (September 1954); almost nothing else on the subject can be found.

Oliver P. Williams and I plan to devote special efforts to the comparative study of interest-group activities in the four middle-sized, nonpartisan, council-manager cities in Michigan that we are studying.

²² Belle Zeller, *American State Legislatures; Report*, New York, Crowell, 1944.

²³ See my article, "Some General Characteristics of Nonpartisan Elections," *American Political Science Review* 46(3): 766-776 (September 1952), where I proposed a number of hypotheses in this connection. These have been tested in some later studies, particularly by Eugene C. Lee in California and by Gordon McKibben in Seattle. I have tried to bring together existing materials within a framework for further research in my article, "A Typology for Nonpartisan Elections" *Western Political Quarterly* 12: 449-458 (June 1959).

ELECTED OFFICIALS

Both the executive and legislative branches of government could profit from study and analysis that are more challenging than a tabulation of the services rendered by legislative reference bureaus in various states. Some serious work is already under way in comparing the function of legislatures and the role of legislators.²⁴ But much of the role of the popularly elected assembly is not known. A study of the relationship of pay levels to the types of persons who become candidates for the council or legislature, for example, could provide important leads concerning the nature of motivation in politics and could be an important guide to policy-making and to the structuring of newspaper editorials. We do not know very much about the meaning of party organization in legislatures, although some work has been done on this, and we know little of the effect upon either party organization or the content of policy where control of the two houses, or of the legislature and the executive, is split. We have been accustomed for so long to think of partisan differences that we have made little inquiry into other differences. One that we have long assumed to exist, however, is that of a rural-urban bifurcation. Yet doubt of sufficient importance to warrant further inquiry has been raised concerning the traditional interpretation of this split.²⁵ And we really know so little about the effects of gerrymandering that bureaus are in no position to advise legislators concerning the implications for representation in various approaches to redistricting.

Some comparative studies of the office of the governor have been published.²⁶ They offer many suggestions and hints of possible further research. The legislative role of governors, mayors, and managers needs to be carefully examined. Some attention has been given to the manager,²⁷ but little to the others. Are governors, despite weak administrative powers, becoming real leaders in policy innovation? What are the techniques used by governors to achieve a leadership role? What is the function of the veto? Why is it conventional for the governors of New York and Louisiana to veto large numbers of "pet" bills each session, but for

²⁴ See Keefe, *op. cit.* A major comparative study of legislators is currently in progress under the direction of Heinz Eulau, William Buchanan, Leroy Ferguson, and John C. Wahlke.

²⁵ See D. R. Durge, "Metropolitan and Outstate Alignments in Illinois and Missouri Legislative Delegations," *American Political Science Review* 52(4): 1051-1065 (December 1958).

²⁶ See Coleman B. Ransone, Jr., *The Office of Governor in the United States*, University, University of Alabama Press, 1956, and Joseph A. Schlesinger, *How They Became Governor*, East Lansing, Michigan State University, Governmental Research Bureau, 1957.

²⁷ See Clarence E. Ridley, *The Role of the City Manager in Policy Formulation*, Chicago, International City Managers Association, 1958.

legislative committees to kill them in Michigan, with few vetoes being used? What are the implications of this for party organization? The governor's role as a policy leader? The public image of the gubernatorial office? Not very much has been done on the changing administrative role of the governor, or on the effect of changes that have taken place upon the quality and levels of service programs, or on changes in budget and policy formulation.²⁸

FINANCE

Certainly one of the vital policy areas in which government-related bureaus often become involved deals with the matter of finance. Many value-related opinions are to be found relative to the merits of the use of state grants-in-aid and shared taxes, but little as to the effect of such financing upon public policy. The usual arguments concerning their effect are well known,²⁹ but what has actually happened under varying conditions of intergovernmental payments has had little systematic inquiry. Continued reliance upon conventional knowledge for making state-local policy decisions regarding the tax base contains a great many dangers, yet the questions that need to be investigated are virtually untouched. Comparative practices and results should be a high priority item, it seems to me.

PAST STUDIES

Metropolitan-area government is today one of the matters of greatest interest to lawmakers, planners, editors, those who control the purse-strings of foundations, and even some of the lay citizens living in metropolitan areas. Comparative studies to date seem to consist largely of the gathering of data on the number of governments in various standard metropolitan areas, the kinds of governmental structures that have been proposed for a supergovernment for such areas, and some economic and social statistics. We still know relatively little about why governments in metropolitan areas have not broken down, to what extent inequities (however defined) do exist, and to what extent they differ from one area to another. We have done almost nothing to determine the order of priorities preferred by citizens in these areas. We have reason to believe they do not regard efficiency as a particularly high value. But how important and in what way is access the central concern? What does the

²⁸ See York Willbern, "Administration in State Governments," *The American Assembly*, *op. cit.*, pp. 111-137, and Siffin, *op. cit.*, for possible areas of research. The whole question of the nature of administrative behavior, widely discussed these days in public administration literature, is a fertile field for comparative studies by bureaus.

²⁹ See R. S. Ford, "State and Local Finance," *The Annals* 266: 15-23 (November 1949) or any textbook on public finance.

citizen mean by access? Within what general limits of values will citizens accept change in structure or service levels in metropolitan areas? Do the existing value patterns relative to government organization in these areas permit, within their scope, a structure that will be able to provide the services demanded by the culture? What kinds of service levels are wanted in what kinds of suburbs? To what degree does the legal "ghettoization" of the suburbs systematically deprive certain groups of a full share of the benefits of the economy and of social equality?³⁰ What are the long-range implications of this?³¹

A comparative study might well be made of metropolitan areas by inquiring into the motivating factors behind the rash of studies that have been conducted in the last decade or so. What are the reasons for so much interest? What are the implicit value assumptions in such studies? The reports are usually couched in terms of service problems, "waste and duplication," and inequities in service levels and tax burdens. While all three of these kinds of conditions probably are common, I would submit the hypothesis for examination that metropolitan-area studies are generally not motivated by concerns about service breakdowns, suspected inequities, or even "waste and duplication," but that they are usually motivated by certain economic and ideological considerations. Specifically, I would like to see a comparative testing of the proposition that studies in the largest metropolitan areas are motivated principally by a concern among business, industrial, and labor leaders that the core city of such areas is economically and sociologically obsolescent and that studies in the smaller metropolitan areas are motivated by a chamber-of-commerce boosterism. The large city studies, because of the motivating factors, are principally concerned with economic and land-use studies. To the leading advocates of the studies, governmental consolidation is not considered necessary or even of much importance. Studies in the smaller standard metropolitan areas, however, center around efforts at governmental reorganization, with consolidation or annexation considerations paramount. These are the communities to which industry is moving today. Rather than feeling a concern about threatened loss of property values, the local leaders feel committed to the community-growth theory. They extend their economic value assumptions to government: Growth is good. Bigness is good. Rational, systematic organization is good. There is no such thing

³⁰ See Edward C. Banfield and Morton Grodzins, *Government and Housing in Metropolitan Areas*, New York, McGraw-Hill, 1958. Charles Press and others in The Institute for Community Development and Services are working on some of these problems.

³¹ See Robert C. Wood, *Suburbia; Its People and Their Politics*, Boston, Houghton Mifflin, 1959, for a budget of suggestions for research in suburban politics and a theory of suburban ideology and political patterns.

as stable prosperity; an area must expand or die. The annexation of new land to the city or consolidation of units is a symbol of progress.

A metropolitan area of any size is, however, a social-economic-political complex that is not too different from a region of the nation in the elaborateness of the interests involved. Few people would argue that because states have arbitrary boundaries, vast differences in wealth, and watersheds and transportation networks that transcend their boundaries, we should abolish them together with the whole concept of federalism. More research and writing is needed to show whether metropolitan areas are made up of a system of interests not unlike states or regions and whether a viable political system is possible despite the fact that metropolitan area governmental arrangements are often not neat.

Other questions may be raised that call for research, too. Can the metropolitan citizen's view of what are important problems be measured in relation to the concerns of functional specialists relative to the same question? What are the most significant factors that will affect future governmental patterns in areas of large population concentration?

These and hundreds of other questions can be examined by university bureaus. Relatively few of them have been inspected even casually. In all cases involving local (as distinct from state and national) governments, comparative studies can be undertaken within the space of a rather small geographic area. Although comparisons between metropolitan areas are needed, much inquiry can be undertaken simply by examining suburbs or by contrasting suburbs with the core city within a single complex. Although sociologists have done quite a bit of this, political scientists have not, except for collecting basic data such as those comparing the ratio of policemen to population. These data are important, of course, but unless analysis follows their gathering, they are meaningless. Certainly in the area of metropolitan-area studies, bureaus have both a strong obligation and excellent opportunities for research. A great deal of data have been gathered. The time has come for some analysis.

CONCLUSIONS

In the first part of this paper, I have summarized some of the history of government-related bureaus and considered it in light of the possibility of doing comparative studies. In the second part, I have suggested some kinds of studies that might be done on a comparative basis. It seems to me that bureaus will need to expand their interests and the scope of their personnel as well as their budgets if they are to perform more than fact-gathering and laying-on-of-hands functions, or if they are to recruit and hold first-rate people in an increasingly tight personnel

market. I have suggested that they add to their staffs persons from fields other than political science and that they deliberately seek to coordinate their work with that of other bureaus. To be able to make progress toward a set of theories relative to the process of state and local government, which I have viewed as necessary before bureaus can confidently offer much advice on public policy, I have suggested that these agencies might work out an area-exchange agreement with two or more other bureaus when investigating in politically sensitive areas.

Since bureaus have traditionally concentrated on state and local government activities, I have used this as the framework for the discussion here. I have not dealt with the possibility of bureaus doing work in other areas, including international comparative studies, although I see no reason why they should not do so, budgets permitting.

Many persons in the social sciences wish to develop a discipline that will one day permit some degree of prediction and, hence, will be able to make meaningful and confident evaluations of proposed public policy. Because of the social environment in which bureaus have existed from the beginning—viewed as advisory research arms for state and local officials and lawmakers—these institutions have special reason to be interested in the development of a theoretical discipline that can provide an analytical framework for the kinds of questions addressed to them. Government-related bureaus, as much or more than any other institutions availing themselves of the services of professional political scientists, have every reason to seek to further a science of politics.

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Comment

IT SEEMS SAFE to assume that the greater proportion of political scientists whose professional interests embrace American state and local government have been concerned, to one degree or another, with the work of the government-related university research bureaus. This is true in particular of the men in the state universities. Those affiliated with the endowed institutions are also involved, however, because grist for their professional mills comes primarily from work undertaken or stimulated by the bureaus. Without the bureaus, basic data on state and local government would be much thinner than it is. Whatever their shortcomings in systematic research, and however pedestrian many of the questions they have studied, they have nonetheless produced a body of material much more substantial and comprehensive than exists for any other local government system in the world.

In his stimulating paper, Professor Adrian's immediate concern—and the focus of these comments—is the question of execution of comparative studies by these bureaus. These are understood to be studies along approximately parallel lines by two or more bureaus, as distinguished from analyses, of a comparative nature but in a single context, performed by individual bureaus. This definition would exclude, for example, a study undertaken by a single bureau of decision-making patterns in city-manager cities within one state. It would include studies of these or other patterns in city-manager cities in several states or areas, studies undertaken by bureaus that are parts of the several scenes. That is, in greatly oversimplified terms, it may be said that comparative studies would seek to identify significant variables influencing governmental experience in different settings. Beyond this, they seek to assess the disparate influences of these variables upon consequences observed or experienced in one or another context. Such studies are valuable in several areas of research emphasis. From the standpoint of basic research, they may, for example, contribute to clearer understanding of the special properties of governmental forms, institutions and processes, such as the city manager, the council elected at large, the performance budget, and the long-range capital plan, as apart from political, cultural, and sociological forces in the special environment studied. When the primary objective of the research effort is reliable guidance to public decision-making, such studies may bring greater specificity and pre-

cision to anticipations of consequences in making choices among alternative means for solution of governmental problems in a particular situation. Immediate concern here is with the value of comparative studies in the work of the government-related research bureaus and with requirements for executing them effectively.

At the outset, it seems appropriate to recall the primary mission of the bureaus. Worth emphasis is the point that they were not established—nor has their effort been justified to university administrations and state legislatures—as research establishments devoted to the refinement of political science as a field of theory and instruction. Rather, they are conceived as valuable vehicles for projection of the developed capacities of the university into the life of the community, the state, and the region. It can probably be said that the primary mission of the government-related bureau is to strengthen public decision-making in the particular public context of which the university is an integral part. The bureau undertakes to do this through a variety of relationships, some closely confidential, others broadly publicized. Whatever the approach, the objectives might be summarized as these:

1. To assist in identification and understanding of significant problems needing public action.
2. To develop adequate informational bases for public action.
3. To clarify alternatives of action and probable implications of available choices.
4. To reinforce the technical reliability of the choice made, so as to minimize possibilities of unanticipated and undesired consequences.
5. To utilize, hopefully, insights gained and experience accumulated toward the building of a more refined, more systematic, more valid theory of the governmental process.

It should also be recognized that primary function varies among bureaus. Differences are matters of emphasis and degree. A realistic way to see these variations is in terms of a range of activity reaching from one extreme of technical reference assistance to the other of long-range, basic studies aimed at refinement of theory and clarification of causal relationships. At intermediate points along this range are types of special, problem-centered analyses, short range in scope and confined by deadlines resulting from the practical necessities of public action. Technical assistance is provided on such matters as air pollution, rezoning, and business taxation. In many bureaus, the daily mail will bring a number of inquiries on action taken by other jurisdictions on such matters, and the legal and administrative intricacies encountered. Problem-centered analyses are the familiar studies usually undertaken on an *ad hoc* basis and often produced in working paper form. They are likely

to deal with relatively immediate questions, such as specific urban fringe area problems, administration of motor vehicle operator licenses, supplementation of state revenues, and effectiveness of alternative administrative controls in public assistance programs. Examples of basic research inquiries are well illustrated by Professor Adrian. Different bureaus show different profiles of activity along this range. Variations result from the nature of needs in the special setting, and the working relationships the bureau has acquired. A characteristic role for the bureau tends to evolve from these and related influences, a role not readily manipulated once clientele expectations have become definite. In other words, the role becomes institutionalized.

When viewed in this context, the importance to the bureaus' role of cross-fertilization through sharing—even pooling—efforts and experience becomes self-evident. It is as important in this clinical area of government as is sharing of experience in medical centers pursuing exploratory work along related, though not necessarily identical, lines. From this sharing of experience comes the leaven for the steady, persistent, patient refinement of any field grounded, as the study of government surely is, in working assumptions of causal relationships. And, of course, this continuous refining of understanding is vital to the utility of reliable service to the real world of public decision-making.

These assumptions regarding the research bureaus are stated to define the context for points and problems mentioned later in these comments. They are not set forth with any intent of drawing issue with Professor Adrian; I am not at all sure that any real difference exists. I may have a stronger feeling than does he of an obligation on the part of the bureaus to relate themselves to their special environments and to reenforce public action so far as the current development of the field permits. This view is not so much an expression of the academic ideal of organized research as it is a candid recognition of the realities. By and large, the bureaus have their roots deep in the environments in which they work. They have acquired accepted roles on the public action scene. Many are key interpreters of the meaning of the university in the life of the state. Some are the primary channels through which legislators, public officials, and civic leaders come to know the university, its resources, and its needs. From these developed relationships flow obligations and limitations, as well as budgetary support and a substantial amount of public prestige. These are the facts of bureau life.

Few of those who participate in government-related analysis (the term research is advisedly reserved) would quarrel with the main points made by Professor Adrian. Nearly all would agree that results of bureau work to date need to be more readily known by, and available to, other

bureaus. Older bureaus, those with twenty-five or more years of experience that have established publication policies, usually also have standing exchange arrangements with other bureaus. To make these materials usable, however, a specialized library in competent and knowledgeable hands is indispensable. Beyond the formal and serialized publications are other materials, such as basic official reports of other jurisdictions. These become bulky and difficult to process; they have only occasional utility. More difficult still are the fragmenta, the occasional issuances of civic groups and other bodies. These also can accumulate to burdensome volume; the question whether to save and file all, or to treat them selectively, is a difficult one to resolve. Most of these materials are more significant as evidence of the points of view of the issuing agencies than as sources of data on the problems they treat. At the far end of the scale of accessibility are occasional and frequently quite significant memorandum materials that are rarely printed or publicized. These are likely to be confidential working documents prepared for official use. Since, by their nature, they tend to be pointed, incisive, and candid, they may have great value as sources of insight and understanding. Unfortunately, their nature also makes difficult the general circulation to other bureaus that would be valuable. All in all, a clearinghouse function is sorely needed.

There would be general agreement also that considerably more systematic research is needed, the basic research that Professor Adrian so cogently discusses. To be sure, a substantial proportion of bureau work is grounded in some set of working assumptions, and these are rooted in theories, however subtle, of causal relationships. But granting the need and desirability of such research leaves unresolved a number of difficult questions. Professor Adrian expresses a degree of optimism that others may be unable to share.

To begin with, there is the troublesome question of the dependability of available research methodology. To put this point in context, a word of explanation is needed. The "action-oriented" bureau, that is, the agency that is called upon to assist in framing the context for public decision-making, has an obligation to its special clienteles to provide as reliable material as circumstances permit. The materials, moreover, must be presented in usable form, in a style and format that decision makers, whether civic groups, official bodies, or administrators, can use. Technical language and difficult concepts will not do, however meaningful they may be to members of the craft. Often most important is reinforcement of the decision-makers in the exercise of judgment that is after all drawn from intimate insight as to feasibility and to common sense concerning degree of need. Most helpful in supporting

well-motivated action is a sense of grasp of efforts undertaken in other jurisdictions in the face of similar needs and a sense of understanding of the results obtained. What has happened elsewhere is regarded by the decision-makers as the one best evidence of probable experience on home premises. This, of course, is a context in which comparative materials are essential, and, at times, a basic research analysis of consequences elsewhere is less important than a convincing expression of satisfactions and dissatisfactions perceived by those most closely affected by the action taken.

In a genuine sense, this is the accumulation of clinical experience. To be sure, the approach is empirical. Often the data are thin. The satisfactions or dissatisfactions of those most directly involved can be canvassed, but the more objective, tangible evidence of consequences eludes precise definition, is hard to come by, and all too often cannot be verified. It might be argued that, for these reasons, the problems are not researchable. Verified truth cannot be established. At this stage, little more than a projection of alternative interpretations is really possible. It can also be argued that the central need is to support the decision-making process by bringing up the reinforcements that will give decision-makers confidence in moving ahead with healthy and resourceful exploration toward problem solution. Such clinical empiricism may be as sound an approach as the state of the art permits. At present, and doubtless for some years ahead, use of basic research approaches would add little precision to the decision-makers' task. In this connection, the role of basic research is something else. It is to follow the consequences of the exercise of judgment, however explained and justified, and to sharpen the estimate of casual relationships that emerge.

Closely related is another problem. Professor Adrian expresses confidence in the capacity of a bureau staff to work at several levels concurrently. Serious difficulties arise in practice. Much of the reference and advisory work the government-related bureaus are called upon to furnish calls for the services of skilled technical specialists, persons who are at once more skilled and specialized than the research-oriented faculty members, and less concerned with the "open-endedness" lurking in the empirical approaches. Experience indicates that a skilled technical service tends to squeeze out basic research. This is partially because of the orientation of resource people, and partially because effective technical guidance brings in more and more patronage; little time or energy remains for research, even if staff members are deeply interested. Of course, it can be argued that a university should not become this kind of technical resource. Its services to state and community should

be a direct by-product of instruction and research. This concern, whatever its merits, is beside the point when a university bureau has grown into a characteristic and highly valued role. In many states, the university bureau is the only resource that has recognized technical competence, objectivity in its attitudes, and reassuring immunity from partisan pressures or interest-group attitudes associated with other agencies. Realistically, there is no retreat that does imply betrayal of public trust. The work must go on.

Under most conditions, it would seem that the greatly needed work in theory construction and validation may have to be done on a related and somewhat parallel basis. The relationship, to draw upon an analogy, might be compared to that between the clinical work of a university medical center, where the central objective is to accumulate significant empirical data in patient therapy aimed at curing illness, and basic medical research, where the effort is to harness all available resources gained from laboratory research, animal experimentation, and post-mortem examinations, as well as clinical data derived from therapy experience, toward development of more effective therapy approaches for future application. If this point has any validity, attention is needed to development of basic research as a parallel and related effort—but not, so it would seem, an effort integrated with technical assistance. This is a challenge of major proportions.

To bring about this kind of basic research, several obvious resources will be needed. Among them are a much more careful documentation of working assumptions used in technical assistance, not the sort of material that can go into publications or official memoranda, but the more sensitive, even confidential, working-paper background. Also important is financing for basic research. The budgetary conventions that support technical assistance may not be elastic enough to extend to basic research. Separate, even outside, financing may be necessary to give this work self-sufficiency and continuity. Then, too, a rather different sort of publication may be indicated, one that preserves confidentiality and anonymity where important and that is not perceived by public groups as undermining or deriding sincere and well-motivated action taken under pressure of circumstances and on the basis of the best available guidance. Finally, attitudes and interests among social scientists on the faculty are crucial. Interest in the world of action is essential. An enthusiasm for close, objective observation of experience is indispensable. Also basic are refinement of research methodology, projection of fresh theoretical schema, and refining of middle-range working assumptions derived from them. This is a large order.

It may appear that these comments have wandered afield from the matter of comparative studies and from a commentary upon Professor Adrian's paper. On the contrary, the intent has been to add emphasis to the essentiality of comparative material and comprehensive studies at several levels. Technical information, to be meaningful in the action scene, needs to draw upon comparable jurisdictions. Problem-centered analyses and reports, to serve their purposes, require the interpretation of comparative experience. Basic research needs the validating tests of parallel studies done in other universities. It would be folly to suggest that there is any easy or direct way to bring about a genuinely significant sharing of efforts among bureaus. The mere canvassing of present practices and exploration of supplementary methods would be a project of some proportions. The need for comparative studies is, however, great and inescapable. With respect to questions raised in the course of technical assistance, studies collating current experience and developments on a comparative basis would be invaluable. A loose-leaf service for government-related bureaus would justify itself if the venture could be underwritten. Problem-centered working papers and *ad hoc* survey reports, to be genuinely useful, need supplementary background annotation to clarify the context in which the study was made and the purpose it was designed to serve. These also need subsequent evaluations to report and analyze apparent consequences. Collaborative inter-bureau studies bringing experience together in comparative form would offer perspective and balance. But these efforts will not meet the need for basic research, nor should they be expected to do so.

On the point of basic research, Professor Adrian, if I understand him correctly, has raised the crucial considerations. Development of significant comparative research will require a very substantial refinement of all intellectual disciplines bearing upon public action, particularly in reconstructing the theory of the governmental process and in fashioning sharper research tools to test theoretical projections. Also, and most important, it will require resourceful, knowledgeable, and imaginative leadership on the part of political scientists. State and local government in the United States is probably the most fruitful laboratory for creative political research in the contemporary world. Its potentialities have scarcely been touched.



WINSTON W. CROUCH

Research in Metropolitan Affairs

INTEREST IN THE STUDY of metropolitan affairs throughout the United States today has become so great and has led to the production of such an extensive literature regarding the various phases of the subject that it will be possible only to touch upon some of the major points in the course of this paper. Although the metropolis is not a new phenomenon in American politics and life, the significance of the metropolitan community has suddenly burst upon a much larger percentage of our people than ever before. The demand to understand this phenomenon has given an encouragement to researchers that they sadly lacked in former years. At various periods, local government and metropolitan affairs have been neglected by those in the learned professions while dramatic interest centered upon national and international problems. During the past few years, however, metropolitan affairs have come out into the limelight, ranking as one of the truly significant areas of research and study that must challenge a wide array of persons concerned with the scene around them.

The systematic study of the metropolis in the United States began in the twenties. In the field of politics and government the names of Charles E. Merriam of the University of Chicago and Paul Studenski of New York stand out as pioneering figures. Merriam influenced a whole generation of researchers. He, and students influenced by him, did significant work in analyzing the vast number of independent or semi-independent units of local government that existed in most major metropolitan areas. From these studies it became clearly evident that a common characteristic of the metropolitan area was the overlap of governing authority of local units of government responsible for affairs in any small segment of the over-all community. Tax and financial resources of these local authorities were also a subject of study and much information on this subject has been compiled. Merriam and his fellow scholars also influenced political scientists to study the relationships between the state government and local governments, especially with reference to the relationship of the state legislature to cities and counties. Part of these studies was the examination of the electoral make-up of legislatures and an analysis of the so-called urban-rural

conflict. From the twenties a major area of study for students of local government involved the legal problems of developing local home rule. The objective of this whole range of studies was to allow local elected officials to make decisions at the local level and to reduce the influence of the state legislature in the making of decisions regarding "local" matters at the "local" level.

Studenski's monumental study of metropolitan organization and the distribution of powers between the various units of local government was the first of several major analyses of the governmental structure of the metropolitan community. It again taught us a great deal about the complexity of the structure, the duplication of powers, and the irrationality of the distribution of authority that existed in the typical American metropolitan community. Many surveys and analyses that have been made since have simply contributed additional detailed evidence to support the original thesis.

In the field of economic research a great start was made during the era of the twenties by the Russell Sage Foundation study of the economic organization of the metropolitan region of New York. This study was not only monumental in proportions but also gave us, for the first time, a detailed analysis of the organization of the economic base upon which a metropolitan community operates. Unfortunately, however, it was not possible to keep this study up to date as changes took place in the metropolitan area of New York. Also, because of the expense involved in such a study, no comparable studies were undertaken in other metropolitan areas. Hence, our knowledge of this phase of the metropolitan community continues to be largely inadequate, although from the New York study it was often projected that other metropolitan areas would undergo a similar development as they reached the maturity of the New York metropolitan region. Studies in urban land economics and in urban transportation economics developed, particularly in the New York and Chicago regions, giving useful insights into various factors influencing growth and development of metropolitan communities.

While students of government and economics were busy in their respective areas, the relatively new subject of sociology began to make its contribution. One of the outstanding figures in this field was Burgess of the University of Chicago. Studies by scholars influenced by Burgess showed us much about the social organization and make-up of the metropolitan community, including age groupings, racial and linguistic groupings, and patterns of residence in the central city of the metropolitan area. These studies also gave us a theory regarding the growth of the central city and the social change that took place as a result.

THE METROPOLIS CHANGES

Social, political, and economic trends in the United States after the close of World War II produced new dimensions to the metropolitan problem. The studies of the twenties and thirties were based upon materials of their times. Although the metropolitan areas of those days were growing steadily, they followed certain rather discernible patterns. The urban growth that began to appear after 1947 has often been described as explosive. Here was a different type of phenomenon than had been studied before.

Other factors beside the new explosive growth of urban areas called for new types of study. The population of the United States has become more mobile. Great shifts of population have been taking place not only from the rural and small-town areas to the big city but movement between big cities became significant. Also there has been a notable mobility of population within the metropolitan area itself. The economic level of urban populations has risen, producing new demands for goods and services and new types of social activity within the urban community. The labor and job markets of the forties and fifties are vastly different from those of the twenties and thirties. In the field of government and politics new means of communication have made an impact on local as well as state and national politics. Also, since the twenties, the state government has assumed a vastly different role in the government of its area and is organized and staffed in a much different manner to serve its people. Development of many new responsibilities, such as state highways, state policing of highway traffic, state regulation and protection of public health, state financing of education, and a variety of other functions, has produced an entirely different kind of state-local relationship than the one that was analyzed so thoroughly and understandingly in an earlier period. Changes in tax policies have placed the states in an even more preponderant position vis-à-vis the local governments than ever before. Local home-rule today means something vastly different than it did in the twenties.

Let me set forth briefly some of the main fields of study that have been pursued in the effort to understand this new phenomenon of explosive metropolitan growth and the role of the metropolitan community in our world of today. The political scientists, influenced partly by their earlier training, have continued to be heavily preoccupied with the subject of metropolitan organization and the legal structure of local government. Much is known about the case law of local home rule. Similarly, we now know a great deal about the tax base and the sources of financial support for local governments. Much work has been

done upon the administrative structure and administrative procedures of local governments as institutions to improve the efficiency of administration in the local units. Also, a great volume of information has been gathered about ways and means to administer the various functions necessary to support life in the metropolitan area: water supply, police protection, fire protection, sanitation, education, recreation. At the same time, many volumes of research literature point out that in a metropolitan area the local units of government, because of their very nature, have limited areas of jurisdiction. Types of governing authorities have been analyzed with great care and perception. Much is known about the existing structures of local governments in the metropolitan areas. When political leadership produces a new form or a new distribution of authority such as took place in metropolitan Toronto or in Dade County, Florida, the students of government are quick to follow and to analyze the operations of these new structures.

The economists possibly have been less concerned with the metropolitan area than have the political scientists or the sociologists. To most economists the public finance of local government and of the metropolitan areas is a closed subject. Legal limitations have established such tight controls upon public finance that economic forces have been reduced to a point at which they cease to interest most economists. More recently, however, interest has been returning in the field of economic theory as it relates to metropolitan growth. Market forces affecting urban development and redevelopment are proving significant for study.

Sociologists have been industriously studying phases of the metropolitan and urban community. Today we know a great deal about the population make-up, age trends, and social groupings. Much systematic work has been done to inform us about the population that makes up a metropolitan community. Much of this information is directly useful to the student of governmental administration and decision-making.

WHAT ARE THE NEEDS?

Problems continue to arise constantly from conditions existing in the metropolitan areas, and constant pressure is exerted on the political leader and the public officer to find solutions to these problems. Many such leaders desire to know more about the phenomena of metropolitan life and metropolitan growth. They join with university scholars in wanting to know the facts more thoroughly before decisions are made—and yet, time is short and pressures are great. Viewed in one perspective, we can confidently say that we have a good bit of information on the metropolis, that we know a great deal about it and what takes place

there. Viewed in a different perspective, however, there is much remaining to be done. In fact, it is not a gross exaggeration to say that we have scarcely begun to really examine the significant features of present-day metropolitan life. This observation becomes all the more plausible when we realize that the most definite constant in metropolitan life today is change. Those things that we knew with a degree of certainty in the twenties have been changed so materially that we need to re-examine them, not only by the aid of new methods of study but also in the light of new knowledge and new attitudes that we have gained. Therefore, the list of research needs is apt to grow in extent and depth rather rapidly.

One of the long and dearly held beliefs about local government has been that the small local unit of government is more democratic than any large unit can possibly be because it is closer to the people. Therefore, it is assumed that there is wider participation by the citizens in the decision-making of the small local unit of government. However, we need to know more about the democratic process in local government as it exists today. Is the small local government more democratic than the larger one? To what extent do the groupings of people that seek to identify issues and to translate attitudes into political action in the small local unit differ from those that focus upon the governing authorities of the larger unit of government? In the large populous metropolitan area, is the citizen actually "closer" to the unit of government whose official building may be a few blocks from his home than to the metropolitan government whose seat may be several miles from his home or from his place of business? To what extent is his involvement in the affairs of the smaller unit significantly different from his involvement in the larger unit? What kinds of groups are concerned with the issues in a metropolitan area? Are decisions made there more affected by the ballot than by representations made by pressure groups and interested parties? What is the structure of the various groups that have developed in metropolitan areas to voice demands and to influence decisions? Are the methods they use to achieve their goals significantly different from those used at other levels of government? In other words, what is the realistic pattern of political decision-making in the modern metropolitan area where people may travel considerable distances from their homes to their place of employment—where they may belong to many associations that have an interest in issues that may be decided at more than one level of government; where communication by telephone outweighs the face-to-face conversation; and where media of mass information such as newspapers, radio, and television penetrate into almost every home and business in the community? Several sig-

nificant studies concerning political leadership, political organization, and decision-making in metropolitan areas have been carried out within the past ten years. However, the information is still fragmentary and insufficient to give us a complete picture of the way in which political decisions are made in metropolitan areas in the United States today.

Public opinion research is another area of fruitful contribution to an understanding of metropolitan life. Several studies have been undertaken in medium-sized metropolitan areas to determine the extent to which residents of an area identify correctly the level of government that supplies them with governmental services. Other studies have been made to try to ascertain what influences the residents of independent suburban communities to isolate themselves from the central city in which they may be employed, or with which they have important economic connections. Similarly, studies have been undertaken to identify the main issues that concern residents of various areas in a metropolitan region. It is probably a safe generalization to say that an accurate understanding of the structure of the metropolitan area is held only by a select few in any one metropolitan community. The vast bulk of the people are not particularly concerned with governmental structure, but rather with the adequacy and the level of services provided. They have little understanding of the actual facts, and their attitudes toward metropolitan government are determined largely by stereotypes that have been repeated over and over again in the public print. The Metropolitan Study Commission in Greater Philadelphia is probably correct in identifying their number-one problem in a metropolitan action program to be the need to acquaint the people of Philadelphia with the facts about the existing structure and its functions. However, preceding any systematic program of public instruction, there must be a systematic gathering of information and organizing of that information meaningfully. Any other effort smacks of propaganda rather than information.

WHAT IS HOME RULE TODAY?

A second large area of research needs lies in the re-examination and study of the concepts of municipal home rule. In a state such as California, we need to re-examine the actual situation to determine the extent to which the state government has reasserted its influence in fields such as health administration, water supply, state highway construction, traffic control, and related subjects. Today the state in its own right deals directly in functions that are highly important for the citizens of this state. Decisions made by state administrative and

legislative officials cannot help but have important impacts upon the economy and the social and political organization of individual communities. The state has developed important relationships with local communities in the exercise of these functions. We require today a redefinition of the broad concept of state-local relations and local home rule in the light of a systematic analysis of present-day conditions. A part of this inquiry might well be in developing the possible structure of a state government that would permit the correlation of the several activities of the state government that affect a metropolitan area. Correlating those decisions and actions at the metropolitan level, "state government" is not necessarily government from Sacramento.

By far the greater amount of research in metropolitan affairs has been concerned with single metropolitan areas. Less attention has been given to identifying the objectives of state participation in the metropolitan affairs of today. When those objectives have been determined, research programs can be more concisely outlined and designed. The following points are merely suggestive of areas for further study. One objective of the state might be to allocate land and water resources between the urban and rural users within the state. A second might be to develop machinery to identify issues and reformulate policies that will adjust the conflicts between local groups in the various metropolitan areas. A third major objective of state participation may well be to develop state organization that will produce a coordinated approach to administering the affairs of a region where the regional approach is necessary. When we have made more progress in defining the goals of state participation, we shall be prepared to mobilize the data that we have and to determine with greater purpose the remaining areas in which we need to develop more systematic information.

The home-rule problem has yet another dimension. In a number of states the doctrine of home rule has come to mean that the state will have no policy with respect to results of the interrelations between local governments in a metropolitan area. Surely, there must be other alternatives to the chaos of intermunicipal conflict in the average metropolitan area than state resumption of its one-time authority. In our constitutional system the states have an inherent responsibility to determine the basic rules by which local governments shall operate.

Yet, an abundant number of illustrations is at hand to indicate that an extreme interpretation of home rule, when applied to the modern metropolitan area, leads to conditions of frustration and blockades the wide determination of solutions to public policies. Is it in the best interests of public policy to permit, under the guise of local home rule, the incorporation of a small area with a small population and low tax

resources, and to permit that incorporated area to use its legal authority to block the determination of policies that may be in the greater interest of the greater number in a rapidly changing metropolitan area? We need to know much more than we know now about the political and economic factors that influence incorporation of municipalities within metropolitan areas and about the exercise of powers by local units of government in their first ten years of existence. We need to know much more about the actual relationships between groups of local officials within a given area. The relationships between law and politics are significant at every turn. We also need to know much more about the actual happenings where fringe territory is annexed to incorporated municipalities. Today in California (or any other state) almost no body of data exists showing the actual experience with annexation or consolidation of territories. Most of the information we do have is fragmentary and has been devoted to one single objective at a time. A more systematic examination of the subject may reveal realistic alternatives for state policy-making. The concepts that govern state legislation today relative to local governments are based upon conditions that existed in the nineteenth century before metropolitan areas existed.

WHAT IS THE ECONOMIC FUNCTION OF THE CENTRAL CITY?

A third area of study that is challenging is the changing role of the central city in the metropolis. We are generally aware that there is a close inter-relationship economically and socially, if not politically, between the central city and its suburban neighbors. We are also generally aware that the central city, by reason of its age and development, is today undergoing considerable change. In most instances, however, we do not have any extensive information about the type or rate of change that the central city is undergoing. The recent study by Dr. Raymond Vernon entitled, "The Changing Economic Function of the Central City" gives us intriguing insights into the types of changes that may be taking place. Central cities, for example, are becoming more sharply distinguished from the suburbs that surround them. Specialization of economic functions appears to be leading to significant changes in the role played by the central city, yet obsolescence is an ever present danger to the central city that must be faced thoughtfully. As our populations in metropolitan areas expand into the agricultural hinterlands beyond our older central cities and displace agriculture, we need to know more about the basic factors that cause this situation. We need to know more about factors affecting land values, trends in development, and—in many states—more about the distribution of

water supply that makes urban life and land development possible. The costs of overcoming obsolescence in our central city may be enormous, yet, at the same time, the costs of supporting the suburban spread with resulting transportation costs may be equally—or more—expensive.

Although we now know a great deal about the characteristics and make-up of the population of our metropolitan areas, we have an inadequate amount of information about the structure of our population as it relates to the social issues that become problems of metropolitan government. So far as I am aware, we also have, at the present time, no systematic studies of population movement in the central areas of our metropolitan regions. Observers are generally aware that there is movement of population within the area, but there is turnover among homeowners as well as renters, but, to date, we know little about what this means. We know less about what affects it and what it may produce for us in the future.

UNIVERSITY BUREAUS AND METROPOLITAN RESEARCH

I have tried in this paper, perhaps, without success, to avoid taking a purely “problems” approach to the study of metropolitan affairs. Metropolitan research that is cast in the “problems” mold is apt to be “culture-bound”; the “problems” become chiefly those seen from the observer’s point of view. Certainly we need a great amount of information about how the metropolitan area actually works before we can identify problems satisfactorily.

The “problems” approach tends to be that advocated by the public policy-makers. Public commissions, mayors, governors, and those who are bent upon achieving an “action” program of metropolitan reorganization are understandably concerned that data be mobilized in such a manner as to indicate probable results of a choice between identified alternatives. Certainly many such policy leaders express on one hand a feeling of dependence upon university research findings and chagrin, on the other, because some thirty years of work in a variety of university research bureaus and *ad hoc* projects throughout the land do not provide readily at hand the data and conclusions for which they are yearning.

University bureaus working on government-related research can scarcely avoid the study of metropolitan affairs. For one thing, the larger number of such bureaus are organized within the framework of state universities and derive at least their basic funds from public sources. Almost all of them are physically located either within a metropolitan area or are not far distant from the predominant metro-

politan area in the state within which the university operates. Bureaus carry traditionally dual roles: They are an integral part of the university's research and education program, yet at the same time they face outwards to the community and the state to translate a portion of the university's resources to the direct service of the outside adult world. Inasmuch as metropolitan affairs embrace a wide range of interests in the community adjacent to the universities, it follows simply that bureaus must be alert to the challenge presented so close at hand.

Needless to say, however, there are numerous important limitations to what university bureaus can do in the study of metropolitan affairs. University bureaus have usually been able to draw onto their staffs only specialists in political science, law, economics, and sociology. Yet engineers, architects, planners, public health specialists, and numerous others are concerned with very important aspects of urban and metropolitan affairs. University organization itself tends to make difficult the mobilization of the full research talent of a campus in a coordinated consideration of a specific topic of study.

University bureaus should be part of and contribute to the educational program of the university, particularly at the graduate level. This will mean that some portion of the staff will be involved in instruction as well as research. This inherent division of time and energies will have an important influence upon the scope and dimension of studies undertaken by them in the metropolitan field. In contrast is the *ad hoc* problem—action-oriented metropolitan study project that is organized to gather data on a specific metropolitan area and report to a governing board within a specified period of time. This latter type of project can more readily recruit the special talents required, specify a series of goals to be achieved, and meet a production schedule that will tend to serve the needs of policy determiners.

Some interesting vistas of university educational policies are opened up by the thought that urban and metropolitan affairs may come to have the same status of priority as agriculture has long had in the land-grant colleges and atomic energy has had in certain universities in recent times. Were this the case, bureaus concerned with government-related research might well become urban experiment stations or metropolitan research laboratories, appropriately geared to graduate education and to research. Considering for the moment a few of the implications of such a development for academia, one may speculate as to whether the traditions of the classroom lecturer and the lone research scholar are more deeply rooted in the social sciences than in the physical and agricultural sciences. Will larger aggregations of academic people working on *organized research* programs be assimilated

smoothly into our social science facilities? Not the least of the question is: Can the university bureau successfully coordinate the research interests of persons trained in several different disciplines and focus them upon a program of research on metropolitan affairs? Thus far, university bureaus that have been concerned with government-related research have been directed chiefly by political scientists, while the economists, sociologists, geographers, and others have gone their own separate ways.

Another aspect of the matter relates to the nationwide competition for trained personnel. As *ad hoc* programs related to metropolitan and urban affairs have increased in number and size, the drain upon university teaching departments and bureau staffs has increased. Agriculture, physics, and chemistry have met similar competition in their fields by incorporating more of their research programs under the university umbrella, although they also continue to lose some of their people to extramural research establishments.

If university bureaus are to do major work in metropolitan affairs, they probably will have to come to a more satisfactory working relationship with the teaching faculty so that maximum effort can be devoted to research for appropriate periods. Too much research work has had to be "tucked in" after the demands of teaching and committee attending have been satisfied. Finally, people from several disciplines who are individually compatible with colleagues from other disciplines will need to be recruited. The political scientist will still be a political scientist, the economist will continue an economist, and the demographer will continue his craft, but only those who are comfortable among the jargons of guilds other than their own can work effectively in organized research programs.



Comment

PROFESSOR CROUCH, in his paper on "Research in Metropolitan Affairs," has actually dealt with two subjects. The first is concerned with the nature and quality of research in metropolitan affairs and the second with the role of bureaus of governmental research in the study of metropolitan problems.

Unquestionably, the most needed and perhaps potentially the most productive area for local government research is that of the metropolitan community. Today, more than two-thirds of our population is concentrated in metropolitan areas. Within the next fifteen years another fifty million people will be added to the metropolitan population. It is in these metropolitan areas that the great part of our economic, cultural, and social life is carried on. Within these areas, local governments must find means and methods of satisfying the increasing demands for services and carrying out expanding programs to promote the general welfare. In the process, local self-government is faced with its greatest challenge and its severest test.

Professor Crouch has made an excellent analysis of the character and kinds of research as well as the types of the studies which have to date dealt with the political phenomena of metropolitan areas. From this analysis one is faced with the conclusion that only the surface has been touched. Perhaps the greatest contribution of these studies results from the fact that they have focused attention upon the existing chaos of organization for metropolitan government and have helped to point up the need for structural and administrative reform.

Research which has been conducted in metropolitan affairs makes certain points reasonably clear. For example, it is obvious we can no longer afford to permit each small jurisdiction in our metropolitan areas the authority to deal separately with conditions of health, safety, sanitation, water supply, sewage disposal, air pollution, crime, and fire protection in a way that may jeopardize the interests of neighboring areas. Neither can we afford to permit individual communities control over land-use developments without reference to the needs of the area as a whole. Studies seem to agree that a sound regional approach necessitates the elimination of many *ad hoc* special districts in suburban areas and the establishment of some kind of metropolitan government which will have political authority coextensive with the entire area. While the

pattern of the emerging metropolitan government is based upon these general principles, it is generally recognized that the plan for any particular area must be somewhat tailor-made in accordance with existing conditions, attitudes, and special problems.

Much of the governmental research in metropolitan affairs has centered around subject matter which is legalistic or factual and relatively tangible. In the background have been charters, enabling legislation, organization patterns, administrative structure, population data, and land-use statistics. In general, it may be said that the common denominator has been attention to the complexity of our governmental structure, the intricacies of intergovernmental relations, and the relative merits of alternative solutions for dealing with the resultant problems. It is not contended that this research is not essential. Obviously, such studies are basic to any formula for improving governmental organization or any action program aimed at problem solution. It is to be noted, however, that little attention has been given to theoretical research, to the re-examination of currently held concepts or political philosophies, or to new techniques for the improvement of the political processes. Research of the future, especially by bureaus of governmental research, should give increased attention to the objective of improving the governmental processes by which citizens can better understand their community and by which persons responsible for political action can interpret the interest of the citizens.

Professor Crouch has very appropriately pointed up the need for studies that will provide better machinery for identifying public issues and for determining public attitudes with respect to various metropolitan problems. Conceivably, such research could be of considerable practical significance in guiding public officials in the realm of decision-making. To aid in this, researchers must devise some means to reduce issues to understandable terms, to generate more public interest with regard to public affairs, and to develop more scientific means to ascertain public opinion. This necessitates devising some means of presenting issues to reflect public reaction in the face of probable consequences of alternative courses of action.

It is quite clear that the election process, at least as now conceived, does not provide adequate machinery for ascertaining attitudes on most major public questions. Perhaps research of the future can develop new techniques for ascertaining attitudes which will have a high degree of reliability and which will also be consistent with our democratic processes. The typical political-opinion surveys of the man in the street are not adequate for this purpose. Attitude studies are valuable only where they are concerned with informed public opinion. This does not

argue necessarily in favor of ignoring a large segment of our voters, but rather suggests studies be made to try to discover machinery which will promote voter interest and understanding and thereby make attitude studies of value. In re-examining our political concepts, study may well be given to the appropriate role of the public official as a leader or a follower in developing public opinion. Clearer concepts should be formulated as to the role of the expert in government and, correspondingly, as to what matters appropriately are to be decided by experts and what matters are to be decided directly, or indirectly, by the electorate.

Furthermore, we have no very clear concepts as to what particular electorate should decide what questions. In this is involved the issue of home rule. This question has taken on new significance in the light of the fractionated government of the metropolitan community and the growing metropolitan consciousness. Research in metropolitan affairs should help us arrive at some basis for determining what matters are of local concern, what are metropolitan in scope, and what are of state-wide interest. With the development of new loyalties and new concepts in metropolitan government, we must devise a new concept of home rule.

One of the most significant contributions in the area of metropolitan research can be through studies which would aid in the identification of goals and objectives with which public policy is concerned. One of the greatest obstacles to effective metropolitan government is that of our seeming inability to determine just where we want to go and what kind of a community we want to live in. For example, we find in almost every metropolitan area, competition between our suburbs and the downtown area of the central city. Also, we find ourselves struggling with the traffic problem but unable to determine whether to plan our cities so that all who wish may move about by private automobile, or whether we should plan them for principal travel by some kind of rapid mass transit. Furthermore, while many of our major cities are feverishly trying to get going on programs for urban renewal, frequently this is without having carefully thought out the question of what kind of a city do we need to build for the future.

In our metropolitan communities we know, for example, that many people place a high value on suburban living where there is open space and freedom to move around, in contrast with the noise, smoke, and congestion of the city. We also know how to build highways which will enable the suburban residents to commute to their places of business. We do not know, however, how far people are willing to drive, what traffic congestion they are willing to accept, or what price they are willing to pay to enjoy this living environment. Neither do we know

to what extent comparable living satisfaction can be attained through well-planned redevelopment programs within the city. Until we are able to make such determinations with a fair degree of reliability, we cannot be certain that we have a sound basis for making political decisions and undertaking political management in many public affairs.

Research on governmental problems in the future must make greater use of the social scientist who can provide accurate scientific data with regard to attitudes, values, and the consequences of alternative courses of action. This does not mean that we should discard currently held political philosophies or practices, nor does it suggest substituting the results of scientific research in the place of elections. It recognizes, however, that many matters which should be known for intelligent political action are not readily ascertainable. If we are to preserve our system of government, political decisions must be based upon popular consent and developed through citizen participating action. Nevertheless, to cope with the complex governmental problems of metropolitan areas there is need for research that will help to reduce issues to understandable terms and facilitate the ascertaining of attitudes and relative values which citizens place upon cultural, social, and economic objectives.

As indicated by Professor Crouch's paper, bureaus of governmental research have not as yet identified themselves as leaders in the field of metropolitan studies. Perhaps this is due in a large part to the fact that many of the research bureaus have acquired certain established service responsibilities which have tended to dictate the character of the research program along service lines. This is particularly so where bureaus are charged with service obligations to leagues of municipalities or other governmental organizations. Furthermore, the fact that bureaus are staffed largely by personnel trained in public administration and the law has tended to give an orientation to bureau interest along the lines of this training.

If new political concepts are to be developed and new political techniques are to be worked out through research, the scope of research interest of bureaus of governmental research must be much broader than that now currently held by most established bureaus. Three possible roles would seem to be open. One would be that of reorientation including the addition of qualified persons to conduct research in the areas of need which have been indicated. Another would be for the bureau to assume a coordinating role in which an attempt would be made to bring to bear all the research forces necessary to provide answers to the metropolitan puzzle. A third possibility would be for the bureau to function simply as one part of a team—coordinated by a

superior authority—which would embrace the whole gamut of metropolitan affairs.

It is entirely possible that as a structure for metropolitan government becomes more crystallized, bureau service programs may be tied to metropolitan governmental organizations and function on this broader scale much as many now do with respect to municipalities. In the meantime, however, a need persists for studies in metropolitan affairs which will facilitate decisions and programs that will eventually lead to the establishment of effective metropolitan government.



Bureau Research and Community Decisions: Some Points of Intercept

SOME POLITICAL SCIENTISTS and many sociologists and psychologists have been busy in recent years in the investigation of power elites, community power structures, conflict in decision-making groups, leadership in decision-making groups, decision-making cliques, the social background of political decision-makers, and a variety of other equally important kinds of inquiries.¹ All of these inquiries seek to advance a frontier of knowledge, and almost all are successful. The inquiries have posed questions that could not be answered by referring to the statute books, by comparing the formal practices of the governing bodies of a number of cities or states, by measuring existing practices against a predetermined list of best practices, by fostering an exchange of information among public officials, by effecting internship arrangements in local or state government, or by making the processes of government understandable to the alert citizen and to the public official himself. In short, the most original, the most fashionable, and the most intellectually stimulating research in decision-making or in community decision-making has had little connection with the day-to-day activities of most university bureaus of public administration or public affairs. The problem to which this paper addresses itself is the location of points of intercept between useful research in community decision-making and the operational role of the bureau. For my purposes, "community

¹ See Richard C. Snyder, "Author's Note," Roland A. Young, *Approaches to the Study of Politics*, Evanston, Ill., Northwestern University Press, 1958, pp. ix-x, for a convenient, selective bibliography on decision-making analysis. See Peter Rossi, "Community Decision-Making," *ibid.*, pp. 363-382, for a summary of a great body of community decision-making literature. See also Conrad Arensberg, "The Community-Study Method," *American Journal of Sociology* 60(2): 109-124 (September 1954).

I have been able to find only one piece published by a university bureau and carried out under a regular bureau program that focuses on any aspect of community decision-making; see Ralph Smuckler and George Belknap, *Leadership and Participation in Urban Political Affairs*, East Lansing, Michigan State University, Governmental Research Bureau, 1956.

decision-making" is the process by which choice is made on a controversial public issue at the local government level.²

THE LIMITS OF BUREAU RESEARCH

At the risk of repeating a set of assumptions that my colleagues take for granted, it is important at the outset to state explicitly some of the elements of the so-called "strong" bureau environment. I concern myself here with the kind of bureau of public administration commonly charged with responsibility for undertaking special studies at the request of public officials, providing a general information service, and developing "a research program" in the field of public administration. The formal charge to my own Institute is a case in point:

The Institute of Government and Public Affairs is responsible for developing campus and in-service programs in public service training; research programs in the field of government and public affairs; advisory services for public officials and agencies; and citizen conferences and courses of current interest in government.³

The bureau so defined does not include agencies like the University of Michigan Institute of Social Research where the initial conception was that of a research organization oriented toward the solution of basic social scientific problems, nor does it include bureaus that are established but not at all equipped to undertake research in any serious sense.

The environmental elements that are important to most of our bureaus are the following:

Service Responsibilities

Bureaus exist to link the universities and the politician or administrator. An essential element in that link is the furnishing of service research findings and of consultation service to the practitioners of politics and administration. The intellectual level of this activity can be as limited as is frequently the case in the teaching of introductory American government to undergraduates or as sophisticated as some graduate seminars in political behavior.⁴ In either event, the bureau

² The definition is designed to suit the needs of the conference in opening for discussion a wide range of problems in urban government. Any attempt to obtain agreement on a definition of "community" would be time-consuming and only peripheral to bureau concerns. Accordingly, "community" and "local government" tend to be fused (or confused) here.

³ *University of Illinois Statutes*, 1958, Sec. 31(a).

⁴ See Charles S. Hyneman, *The Study of Politics*, Urbana, University of Illinois Press, 1959, pp. 6-7, 10-14, for a different, less friendly, and somewhat more common view about bureaus, bureau personnel, and bureau research, expressed in meticulously objective terms.

can no more divest itself of service work than the political science department can divest itself of teaching. The service load, like the teaching load, may vary from one time period to another. In some bureaus, it is an exclusive concern; in some departments, undergraduate teaching is an exclusive concern. Service responsibilities necessarily limit bureau research if only in that they tend to focus energies on problems of state and local government.

Size

In terms of the number of academic people who owe primary allegiance to the bureau, all bureaus are relatively small. Vaughan and Hobbs found that of thirty bureaus questioned, full-time equivalent professional staff averaged 3.8 persons. Bureaus claiming to employ ten or more persons (presumably secretarial as well as professional) showed payroll totals that made it plain that the work force was heavily loaded with graduate students.⁵ It seems fanciful to think in terms of more than five or six academic people, at the most, constituting the full-time strength of any bureau.

Funds

Bureaus are neither starved out nor do they fall into a most favored department category for allocation of funds. At the 1946 Bureau Appraisal Session, Lawrence Durisch suggested that:

Bureaus do not possess funds . . . to handle adequately the appeals for assistance now received.⁶

Orin Nolting announced that he was:

. . . astounded at the extremely modest budgets of some bureaus that are rendering valuable assistance to local officials.⁷

But Harvey Walker saw it somewhat differently:

Legislators and foundations are each year betting larger and larger sums on the eventual effectiveness of the bureaus; indeed, no other research activity in the field of the social sciences has been more successful in getting funds. . . .⁸

The facts lie somewhere in between. The bureaus are neither as impoverished as they appear to be from off-campus, not as rich as they appear to be in the eyes of departmental colleagues. In any event,

⁵ See Donald S. Vaughan and Edward H. Hobbs, *Bureaus of Research*, University, University of Mississippi, Bureau of Public Administration, 1958, pp. 11, 20.

⁶ *A Critical Appraisal of University Bureaus of Public Administration*, Charlottesville, University of Virginia, Bureau of Public Administration, 1948, p. 34.

⁷ *Ibid.*, p. 43.

⁸ *Ibid.*, p. 24.

almost no bureau could undertake an extended non-service field survey without subsidy, but almost every bureau can shake a man loose for a couple of months at a time for a research project in which the bureau believes.

The Bureau Marketplace

Bureaus are unlikely to be able to employ persons trained in disciplines other than political science, law, or public administration. Moreover, in the current market bureaus are at a disadvantage in competing for research oriented political scientists or public administration specialists. The unidisciplinary character of bureaus results from the fact that with a heavy service load and with only a small staff, it is impossible for the most *avant garde* bureau director to think in terms of more than a single sociologist or a single economist. But the competent single sociologist or economist is reluctant because he would be separated from direct departmental affiliation, and because he would be without disciplinary colleagues in the bureau. Either factor would be discouraging. It has been suggested to me that the combination is like sending an Electric Power Stations Minister to a hydroelectric plant in East Kazakhstan.

The underside of the fact that we do not get sociologists, economists, or psychologists as regular staff is that we do have the resources of a university from which to draw expert consultants. But this is only useful when those consultants can be interested in the kinds of problems with which we are dealing. There seems to be specialized help close by, but it is deceptively easy to overestimate the likely interest of scholars from other fields in the regular research work of the bureau. Admittedly, interdisciplinary activities can be developed on special projects, and bureaus may have a chance to publish research findings of other social scientists, but this is outside of the mainstream of bureau research with which we are concerned here. Morris Janowitz's work that grew out of the Detroit Area Study illustrates this point.⁹ Perhaps the unique advantage that accrues to a bureau by virtue of the university tie is that the bureau is enough of a phenomenon to attract the interest of a large segment of the university community. We may get short-run, technical help because we are a curiosity. We are not centers of a diversified group of scholars who stand ready to sacrifice days or weeks pursuing our problems rather than their own problems.

The bureau must also face up to evidence that research-oriented

⁹ See Morris Janowitz, Deil Wright, and William Delaney, *Public Administration and the Public—Perspectives Toward Government in a Metropolitan Community*, Ann Arbor, University of Michigan, Bureau of Government, 1958.

political scientists are not ready to accept bureau affiliation as a likely way to pursue research objectives. One reason for this is that the teaching department has a snob appeal. Another reason is that bureau research has not reflected, in George Spicer's terms:

The constructive imagination, the insight, the spirit of free inquiry and communication, and other resources which we have a right to expect, and usually find in a good university . . .¹⁰

The research scholar has come to accept the idea that he must teach school in order to earn his research time. He does not accept, as an alternative, the bureau life as a condition for research freedom. Plainly, we need to make the bureau a more attractive proposition to the research scholar, and yet not lose sight of the fact that it is a bureau with which we are dealing and not a center for the advanced study of the behavioral sciences.

I do not find the dilemma to be insoluble. One easy answer, of course, is to suggest that the bureaus accept the schedule of the teaching departments, restrict the "service load" (including time for preparation) of a bureau staff member to approximately half of a gentleman's work week, and insist that the other half be preserved for scholarly activity. The trouble with this solution is that it would put us in the same unsatisfactory position that teaching departments are in, i.e., not quite knowing what job we are hiring a man to do.¹¹ The ultimate challenge is to develop an imaginative research program that can be made a part of the day-to-day business of the bureau, a program to which all staff members can contribute to varying extents, and which will at the same time exclude service manuals, citizen information statements, statutory compilations and the like from the definition of research. I think this challenge can be met in part in the general field covered by the term "community decision-making."

The advantage that accrues to bureau activity in regard to empirical research is that the bureau hand is able to deal with administrators, legislators, and decision-makers on a continuing basis, and is often in the position of being on intimate professional terms with these people. This means that bureau personnel are in a position to make inquiries that reflect a "feel" for the situation or activity under study. The bureau can overcome its institutional limitations by taking full advantage of the opportunity inherent in the bureau idea for systematic and prolonged observation. Although the scope of the subject matter that is

¹⁰ *A Critical Appraisal of University Bureaus of Public Administration*, Charlottesville, University of Virginia, Bureau of Public Administration, 1948, p. 34.

¹¹ See Theodore Caplow and Reece McGee, *The Academic Marketplace*, New York, Basic Books, 1958, pp. 82ff, for a discussion of this point.

available for bureau study is limited and is often without glamor, the bureau can deal with it with assurance.

The assurance with which the bureau can deal with a subject can often exceed the assurance that can be felt by a scholar dependent on retrospective interviews or on contrived laboratory experiments. Political scientists who have done empirical research on a participant-observer level will have empathy with Ross Beiler and Thomas Wood, who note, in connection with their study of Metropolitan Politics of Greater Miami, that:

Often momentous developments appear to have sprung from nothing more than a casual conversation of people in strategic positions without relation to any conscious or organized pressures. This, of course, is deceiving. However, we do believe that our rather detailed first-hand acquaintance with these developments in the past decade has been as useful as all of the apparatus of theory and systematic method. The sensitivity to the interplay of factors which is required probably goes beyond the current capacities of the scientific approach, as narrowly construed.¹²

The point should not be overdone. Dante did not have to go to Hell to write the *Inferno*. On the other hand, the methodological merit of Boswell's *Life of Johnson* should be a source of cheer to political scientists in bureaus and institutes. Therefore, without further trespassing on the area reserved for Professor Pinner's paper, let it be stated that the bureau has a built-in-methodological advantage in the study of some kinds of political phenomena.

My ambitions for bureau research in community decision-making are relatively modest. Political scientists and all people interested in public affairs must be interested in voter decisions on policy questions. I propose we have a trial at studying the extent of such decisions, the ways in which they have been and may be influenced, and what their relationship is to the future of what we loosely call "metropolitan government." Bureaus now play important roles in posing alternative paths of action to organized community groups and to organized local government in urban areas. I would regard it as useful and important if the bureaus could simultaneously contribute to an understanding of the decision-making role of what is literally the sovereign people in the resolution of urban issues. In general, I am interested in identifying optimum strategies for achieving policy or structural change in local government. Most of the remainder of this paper is devoted to this subject. In addition, I have a few comments on possible related bureau research on the use of the expert in local government decision-making, on the consequences of

¹² Ross Beiler and Thomas Wood, *Metropolitan Politics of Greater Miami*, unpublished manuscript, Mimeo., pp. 9-10.

the adoption of city manager government, and on organization for decision-making at the local governmental level. These subjects meet the criteria of importance and of bureau competence.

VOTER DECISIONS IN LOCAL GOVERNMENT

The range of local government issues for voter decisions extends from relatively insignificant policy questions to proposals to change the structure of an existing local unit to full-blown plans for metropolitan government. These are illustrated by daylight-saving referenda; city-manager-government referenda; and referenda on rather drastic proposals like those made in Nashville, Dade County, and Seattle. There is no indication that the number or kinds of decisions offered to the voters at the local government level are likely to be reduced or downgraded. Indeed, the illustrations I have offered did not include elections on tax and bond questions which are likely to remain voter decisions even in the unlikely event that metropolitan consolidation should come to be imposed by state legislatures à la Toronto. It would be useful to establish some framework within which it is possible to analyze referendum campaigns. The analysis should be directed to finding a set of model determinants of success or failure in local referendum situations. Because the bureaus are frequently called upon to support, explain, help develop, prepare background material for, or otherwise participate in the activity that culminates in the policy referendum, bureaus are in an especially good position to contribute to both the framework and the analysis.

Voter Decisions and Representative Decisions

Preliminary to posing questions about different kinds of referendum decisions, and especially about the nature of the political activity that may be required in each case, some propositions may be advanced about the differences between decisions made by a legislative body and decisions made by the voters. I do not believe that these differences are as readily testable as are some other questions about referendum decisions, but I think that they deserve our attention.

A first distinction between a legislative decision and a referendum decision is that the latter will not be as likely as the former to depend on logrolling support. The legislative decision will nearly always reflect and include voting participation by those who are without a direct interest in the result but who are willing to trade their votes on some questions for the votes of their colleagues on other specific issues, or for the promise of a vote at some time when it is needed. In the case of a local referendum, however, the voter is unable to make the kind of log-

rolling arrangements his representative might make in the council chamber. The voter is being offered a single question (or several questions on the same ballot) in a secret ballot, and this makes it impractical to trade support. One does not trade something for nothing. In addition, the physical circumstances deter logrolling in that the ordinary citizen does not meet all the other citizens in the cloakroom or on the floor of a legislative chamber. Communication thus becomes a virtually insuperable obstacle to individual vote trading on any substantial basis, even in the unlikely event that referenda are assured on a sufficiently diversified series of issues to raise the logrolling possibility.

Logrolling is also impeded in a referendum vote by the absence of a party label on each side. The political party is a kind of guide to the paths in which it is profitable to roll someone else's log. Whereas the legislator who is disposed to trade his vote can make necessary arrangements through the political party, or the local substitute for a political party, the individual citizen, voting in a referendum, finds no comparable convenience.

In sum, the legislator, like the private voter, will cast his vote in accordance with his perception of his dominant interest. One factor that may be involved in that interest complex for the legislator, however, is the logrolling arrangement developed on the particular issue. The substance of the arrangement is one of the interests pulling and pushing on the legislator. Eventually, these interests are temporarily resolved in a resultant which will indicate the direction of his vote. The fact that this logrolling factor is not a part of the interest complex weighing on the individual voter in a referendum means that techniques for achieving success in a referendum vote must be different from the techniques that may be used to secure a legislative majority. An important element in one decision-making complex is absent from the other.

A second factor differentiating voter decisions from other ways of achieving public policy decisions is that the question for decision, once posed, remains constant. Whether the question grows out of an initiative petition action or is put to referendum because of a constitutional or statutory requirement, it cannot be adjusted, modified, or compromised as the campaign develops. A bond issue referendum that is attacked as excessive cannot be scaled down to attract votes. A metropolitan government proposal cannot be amended to provide added safeguards for suburban areas. A city manager plan referendum cannot be transformed from a plan calling for an at-large council to a plan providing for the election of a council from single-member districts. In short, although proponents of a measure in a representative assembly have numerous opportunities to make the measure more attractive as they observe the

nature of the opposition, this same opportunity is not present in a voter-decision situation. Once the question is posed by the legislature, the school board, the council, the charter board, or other agency involved, then its terms are fixed, and only the passage of time intrudes on a decision on the question as posed. Moreover, there will be a decision, because unlike the decision-making process in a representative assembly, the process of voter decision-making does not allow for barriers in the form of unfriendly committee chairmen, filibusters, quorum calls, or other procedures which make it possible to avoid a direct vote on the issue. By and large, only a judge can stop a referendum vote, and he is more likely to delay it than to kill it entirely.

The characteristics of referenda that I have posited here suggest that such decisions are especially well suited to bureau research efforts given the limitations and advantages set forth earlier. The referendum is peculiarly isolated in a manner that other public policy decision processes are not. It takes place under physical circumstances and in an institutional and legal environment that suggest that the voter will have to decide on the question as first posed, and that a referendum decision is less likely to become commingled with other public policy decisions than is a decision made by a representative assembly. A referendum question bears no formal party label. In part because of the absence of party labels, bureaus are asked to participate in referendum campaigns. In part because these voter decisions at the local level often have to do with the almost routine problems of state and local government—tax rates, form of government, home rule powers, creation of new governmental units—bureaus find it appropriate to participate as presumably expert commentators. The advantages of the participant-observer role that can accrue to bureau operations are well preserved in the study of voter decisions on the local level. None of the limitations imposed by size, funds, or characteristics of personnel stands in the way of carrying out imaginative research programs in this area.

Municipal charter change, whether it involves an all encompassing scheme of metropolitan government or is limited to a changeover from the aldermanic to the city-manager form of government, is a process in which the formal and informal aspects of political institutions are displayed for study much more completely even than they are in referendum campaigns on financial policy issues or in campaigns for office. It seems entirely likely that metropolitanism will bring an increasing number of referenda on charter change involving merger, city-county consolidation, federation or functional transfers. But even without the metropolitan government fad, every month ten or twelve cities are reported to have adopted city manager government. Occasionally there

will also be a footnote reference to a rejection or two or even to an abandonment. Each of these decisions by the voters is preceded by a campaign that exposes the political system of the community. Each such exposure is a point of intercept between useful research in community decision-making and the operational role of the bureaus. A reasonable research goal for the bureau is the identification of a model community environment within which proposals for structural change in government are likely to be approved. Refinements on the model may permit identification of the particular environment most conducive to approval of a metropolitan government proposal, or to a change to or from manager government. On another level, a model should disclose strategy and tactics most likely to lead to success in various structural change situations. Models may also be possible for tax and bond questions and for other voter decisions on policy questions. Unlike elections to office, the time for which is always fixed, local policy referenda can almost always be scheduled by the proponents alone or by negotiation between proponents and opponents. Defensible research conclusions on the subject would, therefore, not only contribute to an understanding of the political system, but might have prompt practical value, a combination entirely in keeping with bureau objectives.

Tentative Generalizations

In building up an inventory of all aspects of community decisions by referendum, it is to be expected that a great number of propositions will present themselves for testing. I suggest a few that may be worth attention.¹³ These deal only with the campaign stage. Research on referendum results, including questions of degree of voter participation by issue, season of the year, neighborhood, et al., would be informative. I regard the latter as a more self-evident research area than the following:

1. Proposals for charter change, including those for metropolitan government and for manager government, are likely, in the campaign stage, to take either the form of a broad challenge to the existing political structure including the conduct of office, or the form of espousal of a relatively narrow technical improvement which is debarred by the existing structure. The selection of one strategy or another affects the outcome of the referendum.

The proponents of change have essentially two choices as to the conduct of their campaign. One alternative is to accept the interest complex which supported the existing government, attempt to detach some of the

¹³ Several of the propositions that follow grow out of a continuing activity in which my colleague Phillip Monypenny and I are seeking to assemble data on the conditions of political and structural change in local government.

groups comprising it from that government, and transfer such support to a new unit. The basis of such a campaign is that charter change would have no adverse consequences to existing interests and that something positive not currently possible would be gained. An argument as to the economy and efficiency of a single metropolitan government is an attempt to carry out such a detachment. Economy in government generally appears as a neutral argument acceptable to all the interests ordinarily engaged in local politics, except those who immediately profit from the absence of economy. In a small city, with small expenditures, the groups who would so profit are few, and once they are identified as having such a non-public interest they will presumably be isolated and ineffective. A strategy of this type minimizes the possible significance of suppliers, city employees, office holders, and party workers who may be neutral on most policy issues but who have an attachment to the existing governmental structure. As Daniel J. Elazar puts it:

For a variety of reasons, they preferred the status quo, since they had adjusted well to it and could see little gain for themselves in changing it. In the Nashville area this group included businessmen who did not want new industry to come to the area, politicians of the old machine whose power was lessened by any change in the governmental pattern that benefitted the extensively involved (or even the middle class in general), underworld elements who had reached a *modus vivendi* with the powers-that-be, position conservatives, who simply preferred the old days and ways and did not want to see them changed.¹⁴

It assumes that their number is so small that they will be swamped by the action of the disinterested citizenry. It ignores the factors of experience and organization which are in favor of the established group. It also ignores the possible identification of much larger numbers of citizens with this key group.

The opposite and bolder strategy is to form a frankly opposition coalition. To do so requires promotion of policies of interest to dissident groups, policies difficult to secure through existing institutions. It means mobilizing a considerable number of persons, an obvious requirement in a referendum campaign. Moreover, it is likely that the group to be moved would be those who had been comparatively inactive. Such a strategy requires considerable sophistication in local politics and a knowledge of diverse community elements. It has the long-run advantage that not only is an organization created for a referendum campaign, but a basis is laid for subsequent campaigns for office and for the election of office holders who would realize the purposes of the new government.

¹⁴ Daniel J. Elazar, *The Defeat of the Metropolitan Government Charter in Nashville and Davidson County, Tennessee*, American Society of Planning Officials, speech delivered at conference May 13, 1959, Mimeo., p. 9.

The statement of the alternatives is a sufficient indication of the limitations of choice available to supporters of structural change. I think the alternatives are largely mutually exclusive, but the strategy may differ in each community involved in a metropolitan government situation.

2. The opponents of charter change may be expected to work effectively to dramatize the threat to the interests of all of those who support the existing governments and their policies, and the uncertainty of these policies in a new or different unit. The threat will be presented in very particular terms. Existing tax rates are made to depend on the continuance of existing units by pointing out that any existing statutory limits which prevent increases are likely to be voided. In a change to manager government, the positions of government employees are said to be in jeopardy since the law usually makes no explicit provision for them. It will be pointed out that the status of license holders of all kinds will be uncertain in any interim period between the elimination of the old government and the constitution of the new one. The status of pension funds will be called into question. In the case of any form of metropolitan government, it will be alleged that property titles will be called into question, or at least defended only with expense since existing governmental units are an element in the title description. Race relations and housing patterns will be said to be subject to change in a new governmental system. If the proposal involves metropolitan government, this campaign to arouse those who identify themselves with the existing government needs to be successful in only one of the units, and an appeal to the solidity of the established institutions of one unit is surely likely.

3. Arguments of the supporters of charter change may be expected to find the most favorable response in those who are least attached by personal or material ties to the local political organizations and to the opposition personalities, in those who are least stirred by sentimental ties to the established unit, in those least insecure against the threat of increased taxes, in those either indifferent to or economically secure against integrated neighborhoods. The arguments for change will appeal most vividly to those responding to abstractions as against those most impressed by the familiarity of existing things.

4. The opposition to change may be expected quickly and effectively to mobilize the machinery of both political parties to get out the faithful voters. This interest of the party organization, of the office holders, is one that is present in all political campaigns. It is usually divided and therefore relatively neutral. But in this kind of campaign where existing political arrangements and understandings are put in jeopardy, it can be

counted with the opposition. The form of action—the referendum in established voting places—provides the channel which is best adapted to effective bipartisan use.

OTHER POINTS OF INTERCEPT

There are at least three other broad issues in connection with community decisions that seem to me to be interesting, important, and especially appropriate for bureau research.

The Outside Expert in Local Decisions

What has been the relationship between the policy maker and the technical consultant in the identification of urban problems and the tentative choice of solutions? How much influence does the choice of a research group have on the ultimate community decision? In the metropolitan field alone, it is easy to point to a variety of patterns of staffing for the research job—Public Administration Service in Miami; Community Studies, Inc., in Dayton; Washington and St. Louis Universities in St. Louis; the University of Illinois Institute of Government in Chicago. In each of these cases, the staff agency was of a different genus, ranging from the *ad hoc* team in St. Louis, to the (hopefully) permanent local group in Dayton, to the permanently established but itinerant group in Miami, to our permanent Illinois group. Similarly, there is a range in terms of a continuing university affiliation.

One of the presumed justifications for bureau existence is that the bureau will furnish skilled, expert, technical help to state and local decision-makers—that the research objectivity of the university will help both the political leader and the civil servant in the effective performance of their duties. If we can lay aside the question of self-interest, there is an important piece of research to be done on the receptivity of local policy makers to expert help, on the areas in which such help is accepted, on the kinds of governments that utilize university and other outside facilities. A characteristic that distinguishes university bureau personnel from personnel of legislative reference services or from personnel of private governmental consulting firms is the opportunity for the bureau people to slough off the anxieties that beset the others, all of whom are part of the same system, and to develop ideas and attitudes independent of the requirements of self-perpetuation. Are there communities in which this possibility for open-ended investigation is pursued? Do the proposals, the clientele, and the “research products” of bureaus differ from those of non-university agencies in comparable activity? What kinds of local conditions result in a decision to use the research facilities of a bureau rather than P.A.S. or J. L. Jacobs? How

extensively do community leaders differ in their evaluation of "outside" research? Most local government officials have less regular staff help than do officials at higher levels. Does this put the bureau consultant in an especially influential position? Is there any discernible change in the calibre or character of community decisions that can be traced to the influence of "non-organization" specialists?¹⁵

Consequences of the Decision for Manager Government

Seven years ago, the Social Science Research Council's Interuniversity Summer Seminar on Political Behavior outlined a research program on council-manager government.¹⁶ Among the hypotheses advanced were that the plan has a depressing effect on local party organizations, on voting, and on political participation; that geographic and demographic conditions encourage the spread of the plan in certain areas faster than in others; and that the plan consistently appeals to some types of interest groups, arouses hostility in others, and indifference in still others. I admit readily that my interest in the questions posed by the S.S.R.C. group has been intensified recently by two Illinois city manager campaigns in which the League of Women Voters was either indifferent or in opposition to adoption of the plan. Moreover, in one of the campaigns the Association of Commerce was indifferent. Does this make any difference, or are the consequences of adoption the same no matter who opposes adoption? Is there an equivalent here of having been "for Roosevelt before Chicago"?

The fact that the hypotheses of the S.S.R.C. seminar have not been systematically verified or denied has not halted the spread of the plan, but it has left many of us in the position of espousing a governmental form about which we really know very little, and of promoting a professional class about which we are similarly uninformed. Norton Long has made a similar point in a sharper fashion by asserting that:

The claim to be able to structure a politics-free administration has meant in practice an assumption of the rectitude and universality of the values of organized administrators.¹⁷

Rectitude and universality aside, what are the values of the organized administrators who make up the city-manager profession?

¹⁵ See Henry Kissinger, "The Policymaker and the Intellectual," *The Reporter*, March 5, 1959, pp. 30-35.

¹⁶ See "Research in Political Behavior," *American Political Science Review* 46(4): 1009-1015 (December 1952).

¹⁷ Carl Friedrich and Seymour Harris, "Recent Theories and Problems of Local Government," *Public Policy* 1958, Cambridge, Harvard Graduate School of Public Administration, 1958, pp. 285-296.

Organization for Decision-Making in Local Government

When situations become issues for decision on the local government level, what is the variety of formal procedures involved in coming to a decision? Do these formal procedures differ with each complicated issue? Do these formal procedures differ in important respects with governmental form, or other variables? Can the formal procedures be arranged on a routine to critical continuum?

Peter Rossi has pointed out that a limitation on the value of field studies of complicated decisions is that they are both complex and unique and that, therefore, generalizations going beyond the specific issue are dangerous.¹⁸ I think that the bureaus can help cut down on the dangers by classifying varieties of organizational patterns in local government. We have a pretty good idea of the kinds of situations that are likely to produce complicated political decisions in urban government. They include as an arbitrary minimum housing and urban renewal, budgeting and taxation, local improvements, and zoning and planning.

It would be a sufficiently useful development, at the beginning, if we could show the *formal* procedures involved in local government decision-making in a variety of complicated areas. In zoning matters, for example, one pattern involves a zoning board of appeals (different from the city council) in formal decisions on zoning variations or exceptions. Zoning ordinance amendments are habitually the prerogative of the city council, but there are at least two formal decision-making patterns, one requiring preliminary consideration by a plan commission, another making no such requirement.

Decision-making organization in the housing and urban renewal field is susceptible to a variety of forms, some involving several agencies formally independent of the corporate authorities, some involving a single agency other than the council, some involving a peripheral agency and a coordinator, some involving only the council and mayor or manager. Where a bond issue is involved, another decision-making participant is added to the group either in the form of a state commission or the voters. Local improvements—streets, sewers, lights—may require neighborhood initiation, engineering department approval, board of local improvement acquiescence, council approval, court confirmation of a special assessment, or some lesser number of these or even some different and far less complex pattern. Budgeting, besides being initially separable in the grand divisions of executive and legislative,

¹⁸ See Rossi, *op. cit.*, p. 378.

commonly involves standing committees, *ad hoc* committees, and compulsory public hearings, or again, a combination of any of these.

Local government decision-making is subject to a far more disorderly array of formal procedures than is comparable formal decision-making at the state level. We are in a position to evaluate aspects of the formal decision-making apparatus at the state level. We cannot be in a comparable position at the community level until we develop a classification of formal organization for decision-making. The development of this taxonomy would have to be a widespread activity to be useful. Data on formal organization for local government decision-making, once assembled, can be used first to weigh the influence of the various formal units involved (e.g., the actual role of the planning commission in zoning matters in a variety of governmental forms and circumstances), and second to afford further improvement in organization theory.

Herbert Simon has argued that organizations are processes for handling complex problems in simplified form and for factoring these problems into parts in such a way that the separate parts can be dealt with by separate organizational units. Hence, the structure of a governmental department is a reflection of the way in which human minds have come to conceive the purposes and problems with which that department is concerned. The structure of an organization mirrors the structure of a problem as that problem is perceived by the humans who are trying to cope with it.¹⁹

The least behaviorally oriented among us can in good conscience contribute to evaluation of the Simon theory by compiling data on formal decision-making organization, data which can be put to traditional uses in advising legislative commissions and charter commissions about comparative practices. The "gone" behaviorists can plainly have a heyday with this kind of a framework. The duality of the appeal may be adequate justification for research efforts, but it also happens that the Simon theory of organization can be extremely important to an understanding of local government.

¹⁹ See "Recent Advances in Organization Theory," *Research Frontiers in Politics and Government*, Washington, The Brookings Institution, 1955, pp. 23-44, especially pp. 31-38.

Comment

WHEN PROFESSOR STEINER sent me a copy of his informative paper he attached a note to the effect that my comments might be addressed to a subject rather less limiting in some respects than his "Bureau Research in Community Affairs." Although this considerate suggestion did not double me up in uncontrollable mirth, I did find it mildly amusing. (My smiles, I hasten to add, were not in any sense derogatory of Professor Steiner.) For much of my adult life I have tried alternately to study and to take part in—or, at least, be active in behalf of—local community affairs, particularly urban planning, land policy, housing, and urban redevelopment and renewal. Lately I have come to the conviction that the crucial point both in urban studies and in action programs, so-called, lies not in the substantive content of any one or more of the fields that make up community affairs, but rather is in the amorphous and poorly explored area of community decision-making, or as I would prefer to call it, policy-formulation.

THE CASE FOR STUDIES OF POLICY-FORMULATION

In very rough outline the argument for this view of things is this: Nearly all persons concerned with community affairs talk unendingly of problems—of water supply and sewage disposal, traffic and transport, congestion, blight and slums, racial and intergroup relations, recreational space and facilities, of schools, juvenile delinquency, land-use controls, public health protection, etc. (Perhaps they stress problems too much and say too little about opportunities and the accomplishments of on-going programs, but no informed person can deny the existence of serious and persistent problems.) When, however, one backs off a little from the problems of his specialty and tries to gain the perspective of some kind of over-view, he can readily see that in his and all other fields there are a number of specialists, experts, knowledgeable students, or administrators—call them what you will. Not one of them, to be sure, is omniscient, and they often disagree among themselves—from conviction and experience as well as from habit. But many of them, abetted and checked by thoughtful local amateurs, surely could show the way to marked improvement in the affairs of their specialization in almost any urban or metropolitan area in the country. Why, then, is their help so slowly and grudgingly sought and their advice so seldom followed?

To this query at least two replies are commonly made. One points to the fact that many of the most serious problems ought to be dealt with over areas larger than the jurisdictions of any existing local government in an area. Governmental action is essential to nearly all basic improvement, and no existing government has the necessary areal and functional scope. The other reply, which does not exclude the first, stresses the fiscal weakness of urban governments. By and large our cities, the greatest aggregations of income-producing power the world has ever seen, are, in fact, governed by units that are fiscal weaklings. *Ergo*, the suit has to be cut to fit the cloth and it constantly binds at many points and places. Over the years these stringencies inculcate a general feeling of hopelessness and defeatism that, except in the presence or imminence of near disaster in some essential activity, makes even moderate and well-conceived programs look like the imaginings of irresponsible babblers. So most urban governments proceed along the old, worn paths with little drive, scant vision, and erratic leadership, and programs of community activities nearly always turn out to be far too little and much too late. In the memorable phrase of an editorial writer on the *Baltimore Sun*, "... the place keeps going to pot."

With due allowance for the element of caricature that seems unavoidable when one deals in generalities in matters of this kind, I suggest we must recognize some cogency in these replies. Again, however, the rejoinder is plain: After all, we do have persons (some of them, in fact, in bureaus of government research) who know more than a little about local government organization in urban areas and about urban government finance. Why are their ideas so often scorned and their suggestions so seldom accepted and acted upon?

The most nearly convincing answer, it seems to me, is that the people who live in urban and metropolitan areas simply have not been able to reach any substantial degree of consensus (no one, I believe, thinks it could be complete and nearly so) on the principal issues of their public affairs. Consequently, programs in community affairs nearly always are weak and fumbling: Relatively small obstacles stall or divert them; proponents of individual reforms fail to join forces against basic difficulties such as inadequate local government structure and fiscal weakness; the recommendations of experts or specialists are seldom taken seriously by more than a handful of enthusiasts; "and enterprises of great pith and moment . . . lose the name of action."

In other words, I suggest that the underlying factor in the weakness characteristic of the community affairs in most metropolitan and sizable urban localities lies in the processes of public policy formulation for these socio-economic areas themselves and, only to a lesser degree, for

many of their subareas or constituent parts.¹ By *processes of public policy formulation* I mean not primarily procedures or machinery or techniques but, as I will try to explain a little farther on, the whole complex cluster of phenomena and activities from widespread apathy toward local public affairs and differences in the perception or definition of issues by various groups, through the attempts at communication across class and group lines and the operations of veto groups, to the leadership roles of strong mayors and the initiating and conducting of referenda.

Anyone who accepts this view of the local (urban) community scene will, I believe, also accept two corollaries. The first is that in trying to study and to explain these processes of policy-formulation one may be getting close to the core of much of the urban political and other urban social life of our times. The other is that the key to deliberate rational change in urban public affairs may be found in this same cluster of phenomena and activities. From these observations it is a very short step to the opinion that the urban or government researcher *qua* scientist and *qua* reformers' aide might meet here in the enjoyment of common labors.

Now it turns out that the difference between Professor Steiner and me on this subject is not essentially that he wishes to go, at least in the discussions of this conference, from the somewhat limiting concept of community decision-making to the wider fields of community affairs, whereas I have been narrowing my primary interest from several of the substantive fields of local affairs to a focus on community decision-making. Rather, my notion of policy-formulation seems to be considerably more inclusive than his conception of community decision-making. Possibly much of what I see as parts of the processes of policy-formulation he would define as substantive community affairs. Furthermore, he rigorously keeps to the bureau focus, and so far I have not. To make some amends on this latter point and to enable Professor Steiner and other conferees to estimate how far apart he and I may be on community decision-making and public policy-formulation, let me now list some

¹ I wish I had the time and space to develop further the case for this opinion. At the very least this would require consideration of alternative views of what the nature of this underlying weakness is and whether, indeed, there is one. I can only ask the reader to believe that I have tried to evaluate other explanations, including the Marxist, the thesis of the effects of rural domination and urban under-representation in state legislatures, and that of lag between technological change and population growth, on one hand, and local government and other institutional evolution or adaptation, on the other. Although I find some merit in most of them as contributory or possibly as intervening factors, I do not see any of them as the underlying factor in the sense that if it were altered and the processes of policy-formulation remained as they now are, we could confidently expect a marked change in the status of local community affairs.

of the studies I think would be necessary to deepen our understanding of it (or them). Then I will hazard a few opinions as to which ones might be undertaken by bureaus of government research.

KINDS OF STUDIES NEEDED

My list of the kinds of studies needed on this front is not exhaustive and the order of listing does not imply any estimate of their relative significance or of their potential usefulness. Rather, the order is very roughly from the kinds of studies that seem to me to have been within or near the purview of bureaus to those that few if any bureaus have attempted and on to still others that most bureau directors, I am sure, would consider well beyond their range of interest, and concern, or competence.

Just one more comment: I am not saying or implying that these possible subjects of study are discrete, independent units in the processes of policy-formulation. Most certainly they are not. It would be possible and desirable to design studies that included many, in fact most of them. On the other hand, each of them could be the focus of an inquiry or research effort. Some of them have been.

In research in public policy-formulation in local community affairs, then, studies are needed on:

1. Law or ordinance making actions by local (as well as by state and federal) legislative bodies.

2. Referenda—see Professor Steiner's discussion.

3. Actions of semi- or quasi-legislative bodies—e.g., school boards, planning commissions.

4. Administrative interpretation—a not-too-accurate and in some ways a misleading phrase—of legislative actions.

5. Judicial interpretation—same comment.

6. Administrative influences on the shaping of legislative actions and referenda, including those of staff agencies so-called—e.g., planning departments, personnel agencies, etc.

7. The influence and roles of professional and semi-pro consultants or advisors of various kinds *re* legislative actions, referenda, administrative and judicial interpretations, pre-action administrative influence—see Professor Steiner's paper.

8. Inter-governmental relations—state-local, federal-local, federal-state-local, fiscal and nonfiscal, formal and informal, agency and professional—as they influence local community practices and policies.

9. The influence and roles of civic or "citizen" associations and councils, both those concerned more or less across the board on com-

munity affairs and those specialized by functional program or area, *re* 1 to 6.

10. The attempts to supplement the orthodox devices of citizen participation in governmental policy-formulation by quasi-official units or organizations—e.g., community and neighborhood councils with public employees as their secretaries as in Kansas City and Baltimore, somewhat similar area or district groups sponsored and encouraged by official city planning or redevelopment agencies in Cleveland, Philadelphia, New Haven, and elsewhere.

11. The influence and roles of “interest groups” and “pressure groups,” again of various kinds and of varying degrees of visibility, *re* 1 to 6.

12. The special characteristics and problems of labor and consumer groups as somewhat untypical examples of 11.

13. The counterparts of 1, 2, 4, 5, 6, 7, 8, 9, 10, and 11 in the functioning of such quasi-public, nongovernmental organizations as community chests and major welfare associations. Tentatively I would suggest that the part-time officers, committee chairmen, and other “actives” in such organizations are roughly the equivalent of a legislative body: The professional staff members are, of course, the administrative arm; the rare direct appeals to the entire membership or body of contributors amount to referenda; and the relatively few big contributors may often perform some of the functions of high courts—i.e., they may pass on some of the actions of officers and staff and say what is and is not “constitutional.”

14. The nature, evolution, roles, and influence of informal groups *re* the actions of governmental and quasi-governmental agencies.

15. The roles and influence of political parties and “nonpartisan” movements in local public affairs.

16. The influences of social classes and groups on various aspects of public affairs—e.g., in the perception and definition of issues, as reference groups in the development of values and the delineation of social roles.

17. The nature of power and its sanctions as well as of influence and its workings in local public affairs.

18. Patterns of leadership and followership in various segments of local public life, including high officials, especially mayors and city managers as one group of putative leaders—largely another way of stating 17.

19. The character, methods, and problems of communication in respect to community affairs, particularly communication across class, group, vocational, and educational lines.

20. All identifiable influences and processes (not limited to those of classes and groups as in 16) having to do with perception of problems, definition of issues, formation of value systems, and life-styles affecting citizen participation in local public affairs or the roles of various groups in them.

21. The processes of assimilation of recent in-migrant groups from rural backgrounds into urban communities as assimilation may affect their attitudes, predispositions, and actions in respect to various sectors of community affairs.

22. Increases in the amount and distribution of leisure time of urban residents as they may affect, either directly or as intervening factors, the attitudes, actions, or other responses of various groups *re* community affairs.

23. The prevalence, roots, and factors in apathy and passivity in respect to local community affairs, including analyses of programs aimed at counteracting these often recognized and regretted characteristics of urban life.

24. Sense of community in urban and metropolitan localities as it relates, directly or indirectly, to local policy-formulation for the larger localities and their constituent parts.

Clearly, this list, in respect to both concepts and language, is a hybrid. But with all its crudities and inadequacies, at least it indicates something of the range of conditions and factors that go into local policy-formulation and, *pari passu*, what some persons consider to be the appropriate, in fact, the necessary scope of research in this area. To appreciate the richness of this study area one may take a list of this kind and another of the commonly recognized fields of urban public affairs and note down promising-looking combinations of kinds of studies within groupings of fields for the different recognized types of urban localities and of their constituent parts—central cities, neighborhoods, suburbs, rural-urban fringe areas, etc. To this he may add the possibilities of comparative studies among various countries of the Western world and among sections or subcultures of this country—the South, Pacific Coast, New England, etc. Even casually done, this kind of exercise would, I believe, convince almost any reasonable person that this is no thin and narrow field but one of enormous potential productiveness that has too long lain fallow.

WHAT SHOULD THE BUREAUS DO?

But how about the bureaus of government research? Two questions seem to me central: Should they attempt to concentrate much of their

resources in this area? If so, how far into it should they try to go and in what directions?

I assume that local community affairs should be one of the major concerns of the bureaus. It is beyond my competence and my assignment to make any even approximate estimate of what proportion of their staff time and money might reasonably be devoted to them. Certainly, however, the bureaus will not be able to cover competently many of the substantive fields of community affairs in their research programs as distinguished from their *ad hoc* fact-gatherings as services to public officials and agencies. They must specialize somewhere and, despite the complexities and difficulties of this field of policy-formulation, it seems to me a more promising and defensible possibility for most of them than concentrating on, say, public recreation or water supply or traffic or intergroup relations or housing or some combinations of these and other fields. In my judgment the only serious competitors would be urban public finance and intergovernmental relations.

How far should they try to go? The answer here depends in considerable measure on one's estimate of the future status of local community affairs and that of research by bureaus of government research. Perhaps my view is a compound of native optimism and ignorance, but I think I can foresee a marked increase in the stature of both. Partly this opinion may be based on the old principle that if you sleep on the floor, you can't fall out of bed. Be that as it may, about a future rise in the recognition and status of community affairs I am quite confident.² About the bureaus I have to be more cautious, but some unsystematic acquaintance for many years past with their personnel and product plus the possibility that their load of hack work may be increasingly shared with other agencies, and the newer currents of research stirring the areas with which they are most closely associated, provide some basis for expecting an upturn in their fortunes.

Certainly, so far they have hardly stepped through the gate to this field. My guess would be that most bureau directors would think that only a few of the listed studies, nearly all of them among the first items on the list, were appropriate undertakings for their staffs. And the studies done or actually under way would make a short list indeed.

I would hope, however, that over the next five years or so the continued examination of the bureaus' roles and potentialities, of which this conference is an example, would lead to some advance in both the volume and range of studies undertaken. Quite possibly the mount-

² See my "Reviews—Great Cities, Great Problems, Great Possibilities?" *Public Administration Review* 18: 332-340 (Autumn 1958), for an indication of some grounds for this opinion.

ing pressures of urbanization on both local and state governments may help make available more funds for research in community affairs. Whether the bureaus will get much of it and, if they do, will use it for the kinds of studies argued for above are other questions, but on both of them my answer would be—they might. Finally, before long, bureau directors, particularly if they take to heart some of Professor Steiner's suggestions in his section on "the bureau marketplace," may well be able to add to their staffs able younger men sympathetic with, initiated in the methods of, and willing to try their hand at some of the kinds of studies in the middle and latter parts of my list. All in all, it seems to me quite possible that the net of these developments, operating in various mixes from bureau to bureau, might be to double approximately the number of kinds of studies on the list that at least some few bureaus would tackle.

Although the possible changes indicated in the preceding paragraph are in no sense inevitable, they are linked to observable developments in bureau and other circles. Maybe they will prove abortive; maybe not. It seems to me, however, that we can identify two other possible lines of action of a rather different order. These would come about only through the initiative and persistence of bureau staffs themselves.

Professor Steiner points out that often "... the bureau is enough of a curiosity to attract the interest of a large segment of the university community. We may get short-run, technical help because we are a curiosity." What I have in mind would be a variant of "short-run, technical help"—in one sense, technical, but not necessarily short-run.

Most of the work done or under way on studies like those in the middle and latter parts of my list has been in the hands of sociologists and, secondarily, of social psychologists. They have developed concepts and research designs as well as techniques. Although the results to date are spotty and uneven, it seems to me probable that this kind of work will go ahead, possibly in increasing volume. To many of the sociologists responsible it does not make much difference whether, for example, they inquire into the ways social classes and groups affect the attitudes of their members toward foreign aid, health insurance, or a proposal for a federated form of local government in a metropolitan area. It would seem to me quite proper for bureau members to "feed" community affairs issues to their colleagues in sociology and elsewhere who may be contemplating such studies. My guess would be that many of them would be quite receptive. Bureau members, of course, could not expect to control these studies, but they probably would get at least useful insights from them, and the way might be prepared for

genuine collaboration later on. And the initial expenditure of time and brain-power by the bureau representative would be small.

(In addition, of course, we now have some younger sociologists and psychologists who are keenly interested in political issues and in what they can do to further understanding of them and of political processes. Some even refer to themselves, at least in some circles, as political sociologists. In my acquaintance are only a few of this breed, but in quality of mind and scholarly promise they are impressive. Perhaps researchers in bureaus and elsewhere ought to be searching them out.)

Finally, why should not a few bureau directors join intellectual and financial resources to provide a reasonably well-paying, full-time research position for a politically oriented sociologist (or a sociologically oriented political scientist) to work on some of these rather unorthodox studies of policy formulation in urban communities? It would not have to be a permanent appointment. A three-year (or, maybe, even a two-year) appointment might be sufficient to make a start. It might attract some sociologist of middle range (i.e., not a novice nor yet an international grand master) from a member institution or elsewhere—a man or woman of demonstrated research capacity who could take the job on leave. The obstacle of isolating a sociologist or social-psychologist in one bureau might be offset, at least partially, by the prospect of contacts with scholars, sociologists, and others, at other institutions as well as with politicians and “men of affairs” in more than one local community.

If all went well, this job might be continued, and a series of really worthwhile case and comparative studies might be turned out by a number of scholars holding the key position for a term of years each.

I would suggest, too, that this kind of arrangement might be particularly good for a now very rare but much-needed type of study. It would be, in effect, a linking-up of several of those on my list—all focussed on one substantial local issue. In other words, some instance of major policy-formulation by a city council or of the electorate could be studied, both before and after the chief step in its evolution. The roles of administrative officials, consultants, pressure groups, parties, and others could be looked into in terms of their perceptions of the issue: The roles of reference groups, the parts played by mass media and by the leaders of informal groups, the difficulties of communication, the identifiable effects of various senses of community, local, group and class loyalties, etc., could be researched.

In saying that studies of this kind are rare, I am aware that some available studies may seem to fit this description. All of them I know, however, really do not. They are too loose and superficial. With due

respect to those who made them, who did their best with the tools and resources at hand, these accounts add little to what anyone with some background in political matters would get from reading a reasonably competent newspaper reporter on the same story.

In hoping that the arrangement suggested might produce notably superior results, I am relying on two considerations. Such a study would benefit from what Professor Steiner points to as the principal advantage of bureaus in empirical research—the “feel” for a situation or activity that comes from continuing, first-hand contact with administrators, legislators, et al. This should be most valuable, particularly in designing the study and in the final rounding-up and interpretation of findings. Perhaps equally significant, however, might be the sociologists’ apparatus of analysis and his command of some of the newer methods of social research. Wedding these qualities and approaches might take some doing, but would, I think, be well worth trying—not once but many times.³

In suggesting this joint bureau set-up I am aware of many difficulties and pitfalls that it might encounter in practice. Quite surely, there are others I do not foresee. It must be up to the conferees and their colleagues to decide whether, where, and in what form it should be tried.

³ Incidentally, in the collaboration between representatives of these two kinds of students of local public affairs, each might be a useful corrective of the most common weakness of the other. The bureau man might prevent the social researcher from scattering too much of his efforts on relatively insignificant or nonsignificant aspects of the process or issue under study. The social researcher might counteract the other’s tendency, on the basis of his more or less casual contacts with administrators, legislators and politicians, to assume the role of “inside dopester.”

Public Policy Studies; an Approach to Governmental Research

A BASIC INTELLECTUAL PROBLEM for the bureaus and institutes of governmental research arises from efforts to apply standards of practicability and of theoretical significance to their work. As agencies within the university, the bureaus and institutes are expected to make significant scholarly contributions to the intellectual life of the university. As agencies organizing intelligence for decision-makers, especially in state and local governments, their work is expected to have a practical relevance for an understanding of current problems in public affairs.

The twin criteria of practicability and theoretical significance should pose no fundamental contradiction. An explicit theoretical framework is an essential tool for both the research scholar and the decision-maker to use in critically analyzing the meaning and significance of the data presented in any research report. Yet the requirements of decision-makers are apt to be sufficiently urgent that any information, however limited in theoretical significance, is welcomed. The pressure of the clientele lends substantial weight to standards of practicability. Without a strong compensating theoretical interest, the work of the bureaus and institutes of governmental research is apt to reflect the application of unquestioned premises and methods of research to an endless variety of problems. The bureaus need to have better criteria for selecting the research studies to be undertaken and for indicating the relevance of the studies which they do undertake. These criteria can be specified only in view of definite theoretical interests. This does not mean that bureaus should commit themselves to a single type of study or make theoretical interests as the exclusive or controlling interest in their work.

THE EARLIER TRADITION

The formation of the governmental research movement was rooted in a highly sophisticated theoretical tradition associated with the rise of scientific management and an increasing confidence in the development of a "pure science" of administration. Scientific management had brought the authority of science and the scientific method to the or-

ganization of production, and advancing technology had created immense opportunities for new scales of economic organization. Reorganization of operations and the scale of enterprise greatly enhanced the efficiency of production. The intellectual excitement of rationalizing administrative organization through scientific management and the substantial economic payoff from increased efficiency through reorganization turned the study of administration into a quest for principles of organization which would constitute a "pure science" or a "pure theory" of administration. Efficiency and economy also became a slogan of the political and administrative reform movements that derived their stimulus from the scientific study of administration.

The doctrine of the new "science" of administration was formulated as prescriptive principles derived from the practices associated with the management of leading examples of large-scale organization including large industrial establishments, the military services, the Roman Catholic church, national governments, and public utilities. The basic theses regarding the dichotomy of politics and administration, unity of command, span of control, the scalar principle and similar propositions provide a rational model applicable to a wide variety of organizational experience.

The principles of administration were an enunciation of the principles of hierarchical organization. Hierarchical organization became the standard reference in the "pure science" of administration in the same way that "pure" competition and "perfect" competition became the models for organization in economic theory. These ideal models were assumed to be valid in the same way that the laws of mechanics could be based upon propositions about motion without reference to friction.

The "pure science" of administration reflected an intellectual tradition in which the scholar created an image of himself as the source of new ideas and basic innovations. Application of basic ideas from a "pure science" to practical problems was the task of "applied" research, but applied research could not advance beyond the fund of knowledge developed by basic research. The state of scientific achievement was assumed to determine the level of accomplishment attainable in the practical arts. The state of progress depended upon the state of the sciences.

This intellectual tradition of the "pure science" of administration provided the conceptual organization for the study of public administration and for the governmental research movement. The task of the administrative survey and the research study was to apply the principles of administration in an analysis of existing organizational arrangement. Conclusions and recommendations were formulated to implement the principles where practice did not conform to the prescription. Forces

might exist which interfered with sound principles of organization, but these obstacles to "sound" organization were like friction in the machine.

In studies of public policy, the "pure science" of administration could provide a theoretical referent only for questions of organizational policy. This was a significant element in governmental research, but the concerns of public policy were obviously not limited to questions of organization. Other problems were generally analyzed without reference to any explicit theoretical system.

The demands for studies of policy problems persist independent of the degree of theoretical sophistication for investigating the problems. As a result a large number of policy studies have been undertaken with primary reference to the experience of other political jurisdictions and an effort to evaluate the relative merits and demerits of the leading proposals for dealing with a problem. These studies have been an important source of information for the decision-maker, but have provided meager contributions to a scholarly understanding of the character of the problems. Uniformly these studies have been concerned with the organization of intelligence preliminary to a decision and rarely, if ever, have been concerned with an analysis of the events which followed from the decision.

As the traditional doctrine of public administration has given way to extensive and systematic criticism, its precepts have not been replaced by any other doctrine that has been able to command such confident adherence. The "pure science" of administration has become a chapter in the intellectual development of the theory of organization. As a result, the governmental research movement is confronted with the task of devising new intellectual strategies for the organization of its work.

POLICIES AS HYPOTHESES

A somewhat different approach to the public policy study may offer possibilities for the organization of research that can meet the dual criteria of practicability and of scholarly significance. This approach was perceived by John Dewey when he observed that:

. . . every measure of policy put into operation is, *logically*, and *should* be actually, of the nature of an experiment. For (1) it represents the adoption of one out of a number of alternative conceptions as possible plans of action, and (2) its execution is followed by consequences which, while not as capable of definite or exclusive differentiation as in the case of physical experimentation, are none the less observable within limits, so they may serve as tests of the validity of the conception acted upon. The idea that because social phenomena do not permit the controlled variation of sets of conditions in a one-by-one series of operations, therefore the experimental method has no application at

all, stands in the way of taking advantage of the experimental method to the extent that is practicable. . . . Recognition of its experimental character would demand, on the side of its contents, that they be rendered as definite as possible in terms of a number of well thought out alternatives, or as members of a disjunctive system. That is, failure to recognize its experimental character encourages treatment of a policy as an isolated independent measure. This relative isolation puts a premium upon formation of policies in a comparatively improvised way, influenced by immediate conditions and pressures rather than by surveys of conditions and consequences. On the other side, failure to take into account the experimental nature of policies undertaken, encourages laxity and discontinuity in discriminative observation of the consequences that result from its adoption. The result is merely that it works or it does not work as a gross whole, and some other policy is then improvised. Lack of careful, selective, continued observation of conditions promotes indefiniteness in formation of policies, and this indefiniteness reacts in turn to obstruct definiteness of the observations relevant to its test and revision.¹

Dewey's suggestion that a policy can be viewed as a hypothesis implies that political experience can serve as a basis for developing and testing generalizations of theoretical or scholarly significance. Both scientific inquiry and political analysis associated with policy-making can function as significant cognitive experiences. Knowledge is an essential condition for effective intervention in the events of the world in order to produce predictable consequences. The events that flow from action based upon knowledge can serve as a test of the conceptions acted upon.

Essentially the same point of view has been expressed by Lasswell, who contends that the manipulative perspective of an administrator seeking to control events in order to produce a predictable state of affairs is as valid grounds for extending knowledge as the scholarly or contemplative perspective of an individual who seeks to understand events by formulating propositions that can predict future states of affairs.²

This conception of the relevance of knowledge as grounds for effective decision-making has led Lasswell to speak of the policy sciences. The policy sciences include all intellectual disciplines or subject matter fields which bear upon problems of policy-formation. This is simply a different way of conceptualizing the array of "sciences," "disciplines," "subjects," "fields," "areas," and "problems" that comprise the elements of intellectual organization in the academic world in a way that is more meaningful to the decision-makers confronting the practical problems of the day.

¹ John Dewey, *Logic, the Theory of Inquiry*, New York, Henry Holt, 1938, pp. 508-509.

² Harold D. Lasswell, "The Scientific Study of International Relations," *The Yearbook of World Affairs* 12: 1-28 (1958).

In thinking of the policy sciences, as such, Lasswell is indicating that the knowledge relevant to the policy-maker cannot be organized as a separate discipline with its own distinct subject matter. A "pure science" of administration cannot be realized, because the function of administration requires knowledge about a variety of conditions being controlled to produce a variety of consequences. In a mental hospital or a school, personalities comprise elements of the system being affected by administrative action and psychology provides essential intelligence in theories of personality and of personality development. In water-resource administration, the hydrologic cycle is being regulated on the basis of knowledge derived from geography, geology, hydraulics, and other "disciplines." All the "subjects" that contribute understanding for the control of events to serve human values form the sciences for policy studies. If concepts do not provide adequate grounds for controlling events, then public experience may provide a negative test of hypothesis and call for the reformulation of the theoretical concepts.

SCIENCE AND POLITICS

The approach to public-policy studies suggested by Dewey and Lasswell requires an explicit understanding of the cognitive experiences involved in the methods of scientific and political inquiry. There are fundamental differences in the two methods of analysis. The scholarly use of the political method to extend the frontiers of human knowledge poses some substantial methodological problems.

In both science and politics, the control of evidence to assure the validity of the concepts being acted upon are equally significant. As a result both fields of endeavor have substantial interests in rules of evidence. Scientific analysis has devoted more attention to the problem of methodology, or the practices which can provide a warranty for evidence, but judicial proceedings and other types of political deliberation have given substantial attention to problems of procedure which serve the same function of controlling evidence. Basic assumptions about the grounds of knowledge and the basis of causation are as significant in the function of the political decision-maker as they are to the work of the scholar. The task of relating the two fields of endeavor requires more than the direct application of scientific methods to political affairs. Yet, the two universes of discourse are intricately related to one another. Science has had a profound effect upon politics in assuring greater control over the conceptions being acted upon. At the same time political experience has significant potential for extending the horizons of knowledge as administrators and politicians become concerned with knowing the grounds on which they are acting and the

meaning of the experience which follows from a course of action. Basic differences of motives, of language, and of strategies emphasize the distinctions in the worlds of scientific and political analysis and discourse.

Motives

An obvious difference in motives relates to the scholar's effort to know and to understand, while the political agent acts to control the events in the world, as these events relate to a community of people. The scholar's knowledge makes prediction and control possible. The political agent's efforts to control may or may not be based upon formal knowledge. Whatever conception a policy-maker acts upon is of interest in the study of public policies. The scholar generally attempts to minimize the degree to which his actions directly impinge upon community affairs. The laboratory and the library give him access to a range of experience which a community could not tolerate in its public affairs. The administrator seeks to realize a state of affairs in the community. He seeks to constrain the potential variety of experience.

While these differences in perspective are fundamental, the scholar still must have recourse to experience to test the validity of his ideas. Valid ideas provide the policy-maker with the knowledge essential to assure his control over events. While the administrator acts only on a limited range of conceptions, his experience, nonetheless, serves as a test of those conceptions which were acted upon. Thus, the motives of the scholar and the policy-maker can remain essentially compatible within the limited range of experimentation feasible in community affairs and within the range of ideas entering into the conception of public action.

Language

The languages of scientific and political discourse are markedly different in making explicit the basic conceptions being acted upon. As a language, science seeks to state the rules or propositions which *describe* regularities in the behavior of natural phenomenon with special emphasis upon the rules of transformation from one state of equilibrium to another. For example, chemistry is not only concerned with oxygen as an element but also is especially interested in characterizing the process of combustion and oxidation by which oxygen is combined with other elements to form a great variety of molecules. The language of political discourse is especially concerned with social policies which state the rules or proposition *prescribing* a relationship among people by ordering their actions and behavior with one another and with their universe.

The range of conceivable variety in human behavior greatly exceeds the range of behavior that can be integrated into a single community. When it becomes necessary for persons to act with the expectation that other persons will behave in a predictable manner, some order must be created out of the variety of behavior that is possible. As a result, laws and policies state the range of permissible behavior to constitute this order among any group of people who are interdependent upon one another. Predictability in human behavior is a necessary condition for the creation of any stable organization. Thus any organization is dependent upon the existence of rules ordering the behavior of those participating in the organization.

Policies are normative or teleological statements in that they attempt to regulate activity to produce preferred or desired consequences. A chemical process such as combustion, for example, may have either beneficial or harmful consequences. Legal statements regarding fire attempt to circumscribe behavior that has harmful consequences while permitting behavior that has beneficial consequences. The ambiguity in policy statements often arises from their negative character seeking to constrain some behavior while permitting variety in other behavior.

The descriptive character of the scientific proposition leads to its classic formulation as an "if . . . , then. . . ." proposition. The dependent clause specifies the conditions; the independent clause indicates the predicted consequences that follow from controlled intervention by the experimenter. Presumably a controlled intervention in a given set of relationships will give rise to one and only one set of consequences of interest to the scholar. The conditions specified should include only those that are both necessary and sufficient to produce the transformation. By these standards, it is then possible to specify the boundaries to the set of events that interact to produce the results.

If one reverses the order of the proposition by specifying the consequence first and then indicating the antecedents that are necessary to produce the consequences, the essential formulation of the policy proposition is indicated. Any given consequence may be produced by several different sets of antecedent conditions. The problem of the policy maker is, in part, to choose the set of alternative strategies that will produce the desired consequence and at the same time optimize other values associated with the course of action.

The basic difference in the two sets of propositions may be clarified by reference to an arithmetic problem. If one accepts certain well-established rules of arithmetic, the solution of the problem of $2 + 2 = 4$ is conclusive. With the specification of conditions, predictable consequences follow in any determinate system. However, if you pose the

problem as follows, "if you want 4, you should . . ." the answer is not so conclusive. There are a large number of conditions that will produce the consequences.

The policy-maker who is seeking to realize certain values or objectives thus must choose from among the alternatives available to him. The scholar cannot make this choice; he can only help to formulate and clarify the problems. The choices will determine the future configuration of community life and the policy-makers have special responsibility to the community that scholars cannot assume. Once any specific rule for realizing the objective is accepted as a basis for action, it becomes instrumental to the consequence and, in a determinant system, becomes fully predictable and knowable.

The basic formulations of the descriptive and prescriptive propositions characteristic of scientific and policy statements indicate the relationship of scientific inquiry to policy-making. Knowledge of nature's regularities, and the conditions under which transformations in natural phenomena occur, creates new possibilities for action in regulating events. Man is able to affect natural processes by changes in conditions or modifications of inputs affecting natural events, and thus produce new sets of possible consequences. To the extent that man can manipulate natural events, he places nature and natural processes under his control. The world is transformed by human intervention in a way that would not exist without human intervention. New possibilities and potentialities for action are created with increasing scientific information.

Extension of knowledge and the new possibilities for action based upon knowledge enhance the problem of choice and of policy-making. The decision-maker can choose his course of action only from the set of possibilities that are available to him. The set of possibilities is derived from his knowledge of nature and the variety and constraints that exist in nature. Thus, every development in scientific understanding has ramifications for practical affairs. As the sets of possibilities are extended new choices are required.

From this point of view the function of science is to expand the set of possibilities, and the function of the polity is to institute an order for action from among the expanding sets of possibilities. The effect of laws and policies is to constrain the possible variety in human behavior, but the choice of the particular course of action that is selected will be affected by the total set of conceivable possibilities. Presumably a decision made from a larger set of possibilities has the prospect of being a better decision, however good is defined, than one made from a more limited set of possibilities. Thus, science is constantly working to transform the task of the decision-maker.

The political decision-maker is intimately dependent upon information derived from scientific inquiry in determining the basic feasibility of his courses of action. Theoretical sophistication in relevant fields of science is an essential source of valid concepts for action even though policy propositions are apt to be somewhat ambiguous in their specific frame of reference. The ambiguity of a policy proposition may lead to confusion about the conceptual grounds for a course of action, unless the problematic situation is reformulated in terms of conditions, hypotheses, and consequences.

Methods

Contrasts in the languages of science and politics are also reflected in basic differences in methods and strategies of inquiry. Science seeks to establish a carefully controlled environment which will enable an observer to deal with the variables of his inquiry under careful scrutiny. The function of the laboratory is precisely to provide a controlled environment allowing only for the specified conditions.

The conditions providing an environment for policy considerations are not subject to such control. The policy-maker must deal with a complex ecological system rather than the controlled environment of the laboratory. Furthermore his actions in any given state of affairs are apt to have a complex set of consequences with a variety of "side-effects." The scholar can take satisfaction in having discovered some basic regularity in nature, but the administrator can hardly afford to dismiss the side-effects or indirect consequences as insignificant. Einstein's formulation for the conversion of mass into energy, for example, is unquestionably one of the most important scientific discoveries in our time, but administrators who are responsible for programs acting on the basis of this conception must be aware of a host of side-effects and indirect consequences which arise from the events flowing from their action.

The multiplicity of consequences which flow from an intervention in an antecedent state of affairs is reflected in the multi-valued character of any system of law. The enabling clauses in any body of law always have to be construed in relation to other clauses which are essentially constraining in nature. Authority to act in regulating a river system, for example, is always constrained by fundamental principles of due process of law requiring the protection of property rights. Legal propositions thus call for a complex conceptual structure ordering the regulation of related sets of consequences to different priorities of values. One set of values in regulating a river system, for example, are subordinated to another set of values regarding property rights. Any policy consideration must be cognizant of the wide range of consequences

flowing from administrative action without regard to the academic exclusion implied in labels of "side-effects" or "indirect consequences." The scholar's relatively narrow frame of reference is apt to place serious limitations upon his analysis of a policy problem.

POLICY STUDIES

This discussion of methods of inquiry indicates some fundamental differences in conceptions and strategies in political and scholarly discourse. However, the differences present no fundamental conflicts or contradictions. Methods of analysis and inquiry in the two fields are essentially complementary. The information derived from scientific inquiry can serve as essential intelligence to the policy-maker and the experience gained in public affairs can serve as a test of the conception being acted upon.

Theoretically significant ideas which can serve as concepts for organizing public conduct are derived from every major scholarly field of endeavor in the university. Astronomy, physics, chemistry, and genetics obviously provide crucial intelligence for the policy-maker in this era of satellites, nuclear energy, and radioactivity. So does practically every other discipline in the physical, biological, and social sciences provide essential intelligence for regulating human conduct in relation to the events of the world.

No single person can hope to embody the universe of information relevant to public policies, but persons concerned with the design of policy studies can at least perceive a range of ideas that may be cogent for policy considerations. With such awareness, the analysis of new ideas and concepts can be undertaken in making the universe of scientific discourse available to policy-makers. Lasswell might well say that the bureaus and institutes of governmental research:

... can be a true center of integration where normative and descriptive frames of reference are simultaneously and continuously applied to the consideration of the policy issues confronting the body politic...³

Failure to recognize the wide universe of theoretically significant ideas relevant to the consideration of public policy will mean that the contribution of the governmental research movement to policy deliberations will necessarily be limited. Specialists in various fields of scientific inquiry, who frequently have little appreciation for the requirements of political analysis and the language of the law, will be called upon to provide some of the essential elements of intelligence for the policy-maker. On the other hand, political scientists, who appreciate the dis-

³ Harold D. Lasswell, "The Political Science of Science," *American Political Science Review* 50(4): 979 (December 1956).

tinctive characteristics and intimate interconnections between scientific discourse and political inquiry, have an important contribution that they can make to practical considerations of public affairs.

Some Possibilities

The policy study approach provides the scholar with an important range of experience for formulating and testing concepts of theoretical significance. Many of the basic problems of the social sciences are not amenable to experimental analysis in a laboratory. As a result, social scientists must turn to the experience of people in the community to test their concepts. Each field in the social sciences should be able to use the hypothetical character of the policy problem to test significant ideas.

In the field of economics, for example, a substantial body of theoretical literature had developed on the character of a competitive economic system and the regulatory function of the market. Increasingly extreme fluctuations in the level of the economy created serious theoretical problems for the classical formulation of the self-regulating capacity of market mechanisms. New formulations, made by Keynes and others, called for public intervention in economic affairs to maintain a more evenly regulated level of economic activity. From the experience derived from acting upon the basis of these conceptions, important new advances are being made in theoretical economics. The pursuit of public policies in regulation of the national economy in the United States or the development of a national economy in India, for example, are contributing essential information about economic behavior, the character of an economic system, and its control and regulation.

The political interests in economic regulation and economic development have been accompanied by the creation of a complex system of analysis in which a wide variety of economic data has been organized to provide indices of economic change. These data are essential to those responsible for the administration of economic controls and is equally essential to those who seek to understand the intricacies of economic behavior. Economics as a science will inevitably become increasingly involved in considerations of economic policy and the experience derived from actions seeking to implement economic policies will become increasingly important for the development of economic theory.

By contrast, American anthropologists in their concern for traditional cultures have failed to take any substantial advantage of the opportunity to study culture change among American Indians as a function of public policies being pursued in relation to the American Indian populations. This lack of theoretical interests in the effect of policy

upon culture change has unquestionably been reflected in a lack of confidence among American policy-makers about the adequacy of the conceptual foundations of American Indian policy.

As a result, policy changes have occurred with such frequency that the Indian population has been deprived of any stable frame of reference about the course of change that may be expected of it. The only stable expectation the American Indians have is that, in time, a new and different policy can be expected to come from Washington. This instability in Indian policies has given rise to a new "reservation" culture of indifference, indecision, and cynicism found among many American Indians.

Anthropologists and political scientists still have a rich opportunity to investigate the complexities of culture change as a function of American Indian policy, at a time when the poverty of historical policies in solving the Indian problem has been thoroughly demonstrated, and efforts to formulate new policies appear to have reached a general impasse. Insights based on this experience in turn may provide valuable guides for American foreign-aid missions working with divergent cultures in distant parts of the world.

The development of the new nations in Africa and Asia provides an extraordinary opportunity for political scientists and sociologists to conceptualize the basic characteristics of social and political systems in relation to the policies necessary to implement a change from village and tribal groupings in a traditional culture to a national society. New institutions must be devised to permit the shaping and sharing of political, economic, respect, and intelligence values by members of the new nation state. Observation of this experience should contribute significantly to our understanding of the processes of government in a modern society.

When the immense scope of the problem of changing peoples from a multiplicity of traditional cultures into a national society is understood, the recourse of native leaders to authoritarian solutions may be more comprehensible. An understanding of the basic changes necessary to develop a new nation is essential intelligence if policy-makers shaping foreign aid plans hope to influence the destiny of the new nations for the free world. Policy studies clarifying the intelligence that can presently be mustered for an understanding of some of these problems in nation-building are of crucial importance, and political scientists can use these opportunities to assess the experience of the new nations and the policies that they are pursuing to provide better theoretical foundations for the study of comparative government.

Richard H. McCleery suggests some of the perspectives and po-

tentialities of a policy-studies approach.⁴ McCleery first considers the conditions existing in an authoritarian prison dominated by security considerations. He then indicates the changes that took place in the "liberal revolution" as an effort to institute a rehabilitation program in the prison. But, the communication and control system of the authoritarian prison was not compatible with efforts to provide opportunities for individual prisoners to seek the assistance of prison staff in dealing with their problems and in the development of new capabilities for meeting problems beyond the prison walls. The study then turns its primary attention to the problem of reorganization and the conceptualization of organization as a communication and control system.

McCleery has made an important contribution to an understanding of the phenomenon of organization by analyzing the changes in communication and decision-making patterns in the course of the reorganization of a prison. His focus was upon the consequences that flowed from reorganization rather than upon the formulation of prescriptions about prison management. As a result his short study contributes more insight into the relation of organization to public policy than all the grand reorganization schemes produced by previous administrative surveys.

Some Elements of a Policy Study

If a study of public policy is to meet standards of theoretical significance as well as practical relevance much greater attention needs to be given to the design and execution of the study. First, the problem involved in a policy question must be formulated so that the events to be controlled can be understood and conceptualized as a system amenable to regulation through alternate courses of action. This is fundamentally a task of theory-construction or model-building. Here relevant ideas can be borrowed from appropriate academic disciplines.

The scope of the model will affect the range of alternatives available for consideration. In the field of smog control, for example, chemical engineers have identified the basic photosynthetic chemical transformations that produce Los Angeles smog from the discharge of hydrocarbons and nitrogen dioxide into the atmosphere. Without means for the control of weather and atmospheric conditions, it is not possible to affect the smog problem directly. As a result it is necessary to extend the boundaries of the planning model and specify the sources of the hydrocarbons and nitrogen dioxide. Some controls can be instituted at this level, but this degree of control is being offset by the general growth

⁴ See Richard H. McCleery, *Policy Change in Prison Management*, East Lansing, Michigan State University, Bureau of Governmental Research, 1957.

of the Southern California economy. This, in turn, leads to other levels of analysis and possibilities for control. Other levels of control would require changes in sources of energy and motive power, modes of transportation, or in patterns of economic development. Thus, chemical transformations, atmospheric conditions, industrial processes, modes of transportation, and economic development all can be conceptualized as a part of the smog-control problem. Public policies depend upon the courses of action available for controlling this sequence of events. Several years of basic investigation have gone into the refinement of the planning model being used by administrators in formulating the smog problem today. The nature of the photosynthetic reaction of nitrogen dioxide and hydrocarbons under atmospheric conditions was discovered only in the course of experience with the Los Angeles smog problem.⁵

A second factor to be considered in a policy study is the capacity of the actor or agency to serve as a regulator in controlling a state of affairs for selected consequences. In the political realm the actor or agent is an essential aspect of the system that must be analyzed in relation to the events which are being controlled. The capacity of the agent to control is essential data bearing upon the regulation of any set of events.

This problem can be illustrated by reference to the regulation of a river system. The hydrographic characteristics of an unregulated river system can be described. The task of regulation can be conceptualized. The consequences of regulation can be specified in its effect upon water yield, power production, fisheries, etc. But the realization of these consequences depends upon the capacity of agents to maintain the regulatory functions. The regulated river then becomes a combined physical-social system in which the hydrographic characteristics of the river are substantially affected by the behavior of the agents maintaining the regulatory functions. Any conceptualization of the possibility of regulating a set of events must consider the capacity of an agent to control the set of events, and any estimate of the consequences of regulation must include reference to the agent's behavior in the regulated system.

The importance of conceptualizing the events to be regulated and the capacity of some agent or agency to control the events was indicated by Police Commissioner Stéphen P. Kennedy when he stated

. . . the real basis [of the growing crime rate in New York City] is conditions over which the police have no control—social, housing, schools, economic conditions.⁶

The Commissioner was indicating that the conception of the law en-

⁵ See A. J. Haagen-Smit, "Air Conservation," *Science* 128: 869-878 (October 17, 1958).

⁶ *Newsweek*, July 27, 1959, p. 30.

forcement problem rests on too narrow grounds and that the agents responsible for law enforcement are not competent to control the broader set of events. Basic reformulations are called for under these circumstances.

A third set of factors that might be included within the scope of a policy study is the redistribution of values that occurs as a result of public action to regulate a set of events. In the interests of economy, a specific community of interests may be specified as a referent system and the investigation of the distributive effects of any program could be confined to that referent system. Any such specification, of course, places arbitrary boundaries upon the scope of the analysis.

In water-resource developments in Southern California, for example, the changes that occurred in making the Owens and Colorado rivers tributary to a carefully regulated local water supply resulted in profound changes in community organization, economic development, and the distribution of wealth. Using Southern California as a referent for appraising the consequences of this action leads to quite different conclusions than if the same events are viewed from the interests of the State of California or the western region of the United States.

An economic analysis of the consequences of any regulatory action can be viewed as cost and benefits in a redistribution of wealth. Questions of who gets what, when, where, and how involve data that can be readily organized for the redistribution of economic values. Similar questions on the redistribution of other basic values including the exercise of power are more difficult to handle. However, any data on the distributive effects of a policy program provide indices which both the decision-maker and the scholar can use in predicting and appraising the consequences which flow from a course of action.

Prospective analyses of policy problems, thus, can make reference to the conditions which give rise to the problem, a conceptualization of the problem itself and the alternative courses of action available for dealing with the problem and a prediction of the consequences likely to follow from action by agents with given capabilities. To test the conceptions being acted upon in pursuance of public policies the analysis, however, must be extended to include an appraisal of the events which follow in the consequent state of affairs that are produced by the action. This is the essential phase that is neglected in most policy studies devoted to the immediate practical requirements of policy-makers. Theoretically significant conclusions can be derived only from competent analysis of the events which follow actions based on policies.

In many fields of endeavor new techniques of measurement will have to be developed in association with new methods of analysis to assure

adequate indices of the changing states of affairs. Measurement and quantification are essential tools to gain accurate control of observation and to assure the organization of competent evidence. Students concerned with the theoretical significance of policy studies need to participate in these methodological developments. Operations research and systems analysis already indicate important possibilities for methodological advances in the study of policy problems.⁷

In conclusion, Dewey's conception of a policy as a hypothesis may provide a research strategy for the bureaus and institutes of governmental research to meet standards of practicability and of theoretical significance in the conduct of their work. The consequences following from the execution of a policy can provide a test of the validity of the concepts being acted upon. The sources of relevant theory are as wide as the university and no special "science" of administration can be expected to suffice as the conceptual foundation for public order in a polity. The scientific enterprise in generally extending the horizons of knowledge also creates new potentialities for action and new grounds for decisions. If the theoretical possibilities of a policy studies program are effectively pursued in the governmental research movement, significant intellectual advances may be expected in all of the "policy sciences" through competent analysis of public experience.

AN APPLICATION TO THE CONFERENCE PROBLEM

The policy study approach, as presented in this paper, can be tested for its utility in formulating the problem of this conference. The bureaus and institutes of governmental research were formed to provide better intelligence for decision-makers in state and local governments. That function can be conceptualized as a policy problem. This conference is concerned with enhancing the scholarly contribution of research performed by the bureaus and institutes of governmental research. Again, this can be conceptualized as a policy problem.

Intelligence and Decision-Making

The provision of the University of Illinois Statutes quoted in Gilbert Y. Steiner's paper indicates that the responsibility of the Institute of Government and Public Affairs is to develop:

1. Campus and in-service programs in public service training.
2. Research programs in the field of government and public affairs.
3. Advisory services for public officials and agencies.
4. Citizen conferences and courses of current interest in government.⁸

⁷ See Roland N. McKean, *Efficiency in Government Through Systems Analysis—With Emphasis on Water Resource Development*, New York, Wiley, 1958.

⁸ *University of Illinois Statutes*, 1958, Sec. 31(a).

This clearly indicates the function of the bureau in contributing to the development and communication of knowledge related to public affairs.

This task can be conceived as requiring the creation of a special information and communication system organized to provide intelligence for citizens and officials as decision-makers. The universities, as centers of learning, are specialized agencies for the organization of information and the communication of knowledge. The bureaus and institutes have the specialized task of giving decision-makers access to the store of knowledge and research skills in a university. Thus, the organization of information or intelligence as reflected in the tradition of the university can be made available to serve the requirements of the decision-maker or the practitioner.

Implied in this relationship is a scale of values giving paramount consideration to the provision of intelligence *services* for governmental agencies, public officials, and citizens. Research and training functions are presumably subordinated as a service function. Where research interests might conflict with the service function, research would be subordinated to the service requirements. On the basis of these considerations, a scale of values placing public service, first; research, second; and teaching, third, might be specified as the scale of values controlling in the usual pattern of bureau organization.

If a comparable scale of values were to be specified for universities generally, the order would probably be teaching, first; research, second; and public service as third. This divergence in scales of value obviously reflects the conflict over the role of an institute or bureau within the university. University faculties and teaching departments are apt to judge the performance of the institutes and bureaus by applying their own scale of values to bureau operations. The conflicting scale of values is also apt to be reflected in personnel problems. Persons interested in an academic career are apt to reflect the teaching-research-public service scale of values rather than the public service-research-teaching scale.

The organization of the institutes and bureaus of research also will reflect the basic scale of values represented in their function. The strong public service orientation is often indicated by special administrative arrangements with the bureaus and institutes reporting directly to a central administrative official in the university rather than to teaching departments or divisions of the university. Advisory committees representative of public service values are apt to predominate in bureau policy considerations.

Greater emphasis will be placed upon longevity in tenure within the service-oriented bureau than within the research-and-teaching-oriented bureau. Officials in state and local public agencies need sufficient stabil-

ity in bureau personnel to permit some person to provide a familiar channel of communication and access to bureau and university resources. Since the client's requirements must be controlling in the research production of a service agency, the bureaus will maintain close control over research schedules. In teaching departments, by contrast, the rigidity of the teaching schedule requires that research production be subordinated. As a result, schedules for research production in a teaching university normally require substantial flexibility.

The establishment of institutes and bureaus of governmental research to provide greater intelligence services for state and local government decision-makers should, theoretically, be subject to appraisal in some degree of detail. If the bureaus and institutes make a difference, this difference should be reflected in information levels. Decision-makers with access to the information provided by bureaus should have different levels of information than decision-makers who do not have such access.

These differences should also reflect variety in the types of information available to decision-makers. This information might reflect specialized knowledge derived from studies of practices and performance by other comparable units of government, or the information might reflect new ideas and intellectual currents developing among university disciplines that may have some general significance in understanding the problems of state and local governments.

Bureau performance might also be reflected in measures of the rate of diffusion of information among state and local government officials. Those who have access to bureau services, again, would be expected to learn of new developments more quickly than the state and local officials who do not have such facilities available.

In general, greater knowledge about problems of government should be reflected in greater development of the art of government by those who have access to this knowledge. It is highly doubtful that this result is consistently attained. Thus, the bureaus might have been in a most advantageous position to analyze the role of information in decision-making situations and to specify those situations where additional information is effectively utilized as against those situations where additional information has a disruptive effect and is often repressed by the decision-makers themselves. This is a critical problem in decision theory and significant theoretical developments in this area might have occurred if those who pursued public policies seeking to enhance intelligence functions for decision-makers had been concerned with understanding the conceptions they were acting upon. Every bureau director, unquestionably, possesses a good deal of personal insight about the role

of information in decision-making and could recount a variety of instances and episodes that might bear upon this question. However, those insights and episodes need to be generalized before they can contribute to an advance in decision theory.

In conclusion, the possibility of conceptualizing the function of the institutes and bureaus of research in terms of a policy to enhance the intelligence available for decision-makers, the appraisal of the consequences of the policy, and the analysis of the experience for a consideration of the role of information in decision theory indicate some of the practical and scholarly potentials of a policy study.

Reorganization

This conference is more immediately concerned with the problem of increasing the scholarly contribution of the research produced by the institutes and bureaus of governmental research. As a policy matter this can be conceptualized as a problem in reorganization associated with a modification of the scale of values reflected in bureau work.

One possibility would be to modify the organization to reflect a scale of values placing research in a paramount position with teaching and public service subordinated in that order. A basic change in personnel and personnel policies as well as in organizational arrangements would be required to accomplish this shift in the scale of values of a bureau or institute of governmental research. Personnel devoted to the priority of research and teaching in their personal scale of values would obviously facilitate such a change. The symbolic function served by the long tenure of a bureau director in a service organization would not be as important in a research organization concerned with the scholarly significance of its work. Change in bureau directors might enhance the variety and range of problems considered worthy of exploration, and more frequent changes in bureau directors might be desired in an organization that was primarily concerned with the scholarly significance of its research production. Similar changes in the scale of values reflected in consultative and control arrangements should be considered in any such reorganization scheme.

A reorganization placing priority upon research and teaching over service values would be expected to produce greater harmony in bureau-university relations, but the subordination of service values might lead to serious disruption of relations with public officials. Many bureaus would not be prepared to sacrifice service functions at the risk of disrupting relationships with their public service clientele.

The alternative of seeking to enhance the scholarly significance of research without, at the same time, being willing to subordinate service

functions is apt to be the situation confronting many universities in the operation of the bureaus and institutes of governmental research. This is a more difficult problem to solve than a reorganization of a bureau's program based upon a clear-cut alteration in the priority of values. Increasing research productivity while maintaining the priority of service functions can at least be approached by giving direction of such a program to personnel who have personal preferences for scholarly research but who recognize the legitimacy of the service program. Such personnel may be able to resolve the conflict over priorities by inventing ways for optimizing research productivity while continuing to perform service functions. This places the burden for resolving the conflicts upon the personnel of the bureau in a way that will allow them to seek opportunities for greater scholarly production while continuing to recognize high priority for service functions.

The reorganization of a bureau to enhance scholarly research production could be analyzed as a problem in policy-development. If the expected consequences flow from a basic modification in the scale of values, the scholarly research production should increase. Closer ties with the university community and greater ease in recruiting university personnel might be anticipated along with some loss in the frequency and closeness of relationships with officials in state and local government agencies. An effort to increase research production without a change in the priority of service functions could be expected to place greater internal stress upon the personnel of the bureau. To the extent that they were able to cope with the problem new ways might be developed for relating basic research to service functions.

Since this problem of enhancing the research productivity of bureaus is essentially a problem in organization and reorganization, the experience gained from efforts at reorganization should serve to clarify concepts about the nature of organization. This, of course, leads directly to problems in the theory of organization. Thus, experience in implementing policies of administrative reorganization should be the source of experience that could be related to basic problems in organization theory.

Comment

OSTROM'S PAPER ADVANCES the thesis that bureaus of government research may pursue a dual purpose. They may aim both for practicality and for theoretical significance. They may aid the policy-maker and at the same time develop science. The link between purposes is knowledge. Knowledge is the goal of science; it is also the essential condition for assistance on policy. The methods of analysis in policy-consideration, which forms the language of political discourse, and in extension of knowledge, which forms the language of science, are complimentary. The links here are dependence upon experience and the use of the experimental method for deriving its lessons.

The thesis may be comforting to the bureau director who has thought he faced a dilemma. And the thesis may be helpful to him if he can convince his university president that it is indeed true.

The thesis provides guidance for the bureau director in choice of his projects. He would avoid those which "reflect the application of unquestioned premises and methods of re-research to an endless variety of problems." He would thus cease to be an errand boy carrying the same luggage all the time. He would define some definite theoretical interests and choose projects to validate these. And project design would become more of a challenge for it would have to serve the two purposes of defining the maximum range of possibilities for the policy-maker and the premises to be tested. Whether such new confrontations would make the bureau director feel that he was emancipated or further burdened, I am not sure.

Ostrom is enough of a realist to see limitations on attainment of either objective. He refers to "the limited range of experimentation feasible in community affairs," which obviously constitutes a limit both for policy-making and for extension of knowledge. He discusses the lack of ability to control the environment and the multiplicity of consequences which account must be taken. These things, of course, are familiar enough to the policy-maker; Ostrom does not, by assuming that there can be no science different from the physical sciences, accept this as reason for defeat.

Ostrom also recognizes the difficulties arising from the fact that we do not have a policy science but rather a number of policy fields and a large number of fragmented bits of learning related thereto. He thinks

we have gotten too sophisticated to find a unifying core in the principles of public administration and, moreover, that these provided "a theoretical referent only for questions of organizational policy." In policy studies referents must be found in the concepts—and use made of the descriptive data—of the many branches of learning.

I propose to add some comments on the practical problems arising from this diffusion of knowledge and then to add a few comments particularly related to the development of our store of knowledge.

A partial answer to the problem of diffusion, and one to which Ostrom refers, is broadened awareness. A wide awareness of what Lasswell calls "normative and descriptive frames of reference" and of some of the substance of data in all of the social sciences—and, to an extent in all fields of learning—is the basic essential for a practitioner in social science.

Second, how far do we find an answer in knowledge as art rather than as substance? The social scientist working in policy fields acquires technical skill comparable to that of the chemist in his laboratory. He learns how to define a problem, outline alternatives, and indicate probable consequences. He acquires habits of thoroughness and objectivity and a feel for limits and for possibilities. How far will this art serve as a bridge from one storehouse of usable information to another?

The bureau is, of course, dependent upon the educational system for creating wide awareness and for developing skill for the social laboratory. At the same time, the bureau should cultivate these things by its own orientation and habits and serve as a stimulant for greater educational effort toward competence for policy-analysis.

The bureau director cannot assume that broad awareness and technical skill will give competence for aid in all aspects of policy in all fields. Ostrom notes the limits on any one person's knowledge. Bureaus of government may build staffs composed of persons with complementary knowledge and skills. They may add other persons to serve on special task forces. But no bureau can duplicate an organization like the Legislative Reference Service of the Library of Congress, which works across the board on policy problems. But this is not necessary. Each Bureau need not duplicate the competence and interests of the others. Each may define its area of theoretical interest and except for bread-and-butter jobs taken on in response to demands of its clientele select areas of policy-analysis which will be useful both to the policy-maker and for testing its theoretical assumptions.

Third, what is the significance of the word "government" in the designation of a bureau in defining what part of the area of knowledge it has responsibility in? Ostrom says that "Political scientists, who appre-

ciate the distinctive characteristics and intimate interconnections between scientific discourse and political inquiry, have an important contribution that they can make to practical considerations of public affairs." Those of us who carry the designation of political scientist will hope he is correct. But the question remains: what is the area in which the political scientist may make his contribution? There is his traditional field of public policy with respect to governmental organization and operations, and authority and freedom, including such things as representation, voting rights, executive powers, freedom of speech, etc. To many persons such issues of policy will appear to be quite enough for consideration by one group of specialists. But in addition to policies of people with respect to government, there are policies of government with respect to people's needs. What is the political scientist's sphere of responsibility with respect to these, including such things as use of water resources, economic policy, educational policy, etc.? Should the political scientist, or specifically a bureau of governmental research, be interested in studying the effects of an increase in interest rates on employment? The answer is obviously "No." What then should be his interest as a researcher on economic policy? Does his legitimate interest flow out of his competence? And does his competence relate to such things as the effects of a government undertaking on politics and the effects of politics on it, the means of policy-making and policy-execution, the effects upon freedom and other values?

I think this question of limitation and definition belongs on the agenda of study for the bureaus, as well as for those who direct graduate projects. Perhaps those of us in political science who have been doing research in public policy have been too fuzzy on this matter, and perhaps there is danger that the current tendency to fuse everything into one great master behavioral science will contribute to further fuzziness.

Fourth, how far do we find in the science of public administration some guide for a practical area of service, both in aid to policy-makers and in development of knowledge, by the bureau of government research? We can admit that public administration was initially defined too narrowly, that its standards of judgment were stated too dogmatically, and that administrative theory alone will not suffice, as Ostrom says, as "the conceptual foundation for public order in a polity." Yet there are present frames of reference in public administration that are useful in economic- and social-policy-determination, and others may be developed with the further extension of knowledge. The concepts of administrative theory served well for the earlier orientation of the work of the bureaus, and it may be that the bureaus can still serve a useful function by concentrating much attention on the practical application

and the experimental testing of the frames of reference embodied in administrative theory. For it is still true that questions of organization, operation, and relationships of administrative structures are important factors in satisfactory policy-determination.

My final comments relate to certain aspects of the development of our store of knowledge. The bureau may pursue various routes to knowledge. Specific policy studies adapted to the immediate needs of the policy-maker may serve as means of testing old assumptions and of suggesting new lines of theoretical study. The scholar who undertakes these is likely to find his horizons broadened, his interests humanized, his feel for reality intensified, and his habits of accuracy and thoroughness strengthened. But other types of study will contribute to the storehouse of knowledge and the scope of the scholar's awareness. The case study that merely lays bare a piece of reality in a policy field is one useful approach. The background study that enlarges the knowledge of ecology or that looks for new theoretical concepts may have immeasurable utility for the design of specific policy studies. A bureau could well be proud if it could produce a study like Dahl and Lindblom, *Politics, Economics, and Welfare*, which enlarged the descriptive frames of reference under which we approach questions of economic policy, or a book like David Truman, *The Governmental Process*, which extended our knowledge both of normative and descriptive standards. Such studies broaden the scholar's grasp of possibilities and limitations in policy-decision. It may even be that bureaus can give attention, not only to physical and institutional factors, but to moral ecologies—to the cultural values that in part determine the utilities which the policy-maker will seek and thus the boundaries within which his decision is made. For the policy-maker, whether he realizes it or not, makes moral choices. And the political scientist should be able to help him sharpen his understanding of his value judgments and of the relation of these to his choices. Freedom, for example, is an objective of policy whose implications and variations need clarification in specific situations.

These observations, like Ostrom's, assume that the bureaus of government will engage in policy studies, and that they should have the dual objective of practicality and theoretical significance in devising these studies. They suggest also that the bureaus, though taking advantage of all opportunities for broadened awareness and transferability of skill, should nevertheless restrict their undertakings to things on which political scientists, including students of public administration, can offer some assurance of competence, but that within their own field of endeavor all means by which knowledge is gained will enhance the quality of policy studies.

Notes on Method in Social and Political Research

THE TERM "METHODOLOGY" currently has a partisan ring among political scientists. A rather heterogeneous congregation of innovators—inappropriately lumped under the single appellation "behaviorism"—has for some time been agitating for more refinement and discrimination in the discipline's intellectual procedures and research technologies. Indeed, this concern with method is perhaps the only interest common to all "behaviorists." Reactions to this insistence on methodological matters have varied from encouragement to heated rejection; not the least common among these responses is a certain wariness lest we exhaust ourselves in sterile disputations and neglect the tasks at hand—whatever these may be.

Yet, the frequent connections between methodological debates and important intellectual advances are unmistakable. All of natural science evolved in the atmosphere of a debate, extending over centuries, between the proponents of empiricism and rationalism; neither team won, but both abstract mathematics and experimental science were led to ever new discoveries and are now so closely intertwined and mutually dependent as to form a single body of knowledge. Similarly, the physics of relativity and of quantum mechanics—to give a recent example—doubtless owe an intellectual debt to the methodological debate over Poincaré's conventionalism and Mach's positive empiricism.

Traditional psychology was jolted, in the first part of the century, by the double onslaught of behaviorism and psychoanalysis, both being, in their earliest formulations, little more than programs for inquiry; the raging arguments between experimentalists and clinicians have now died down, and we have several still separate but equal branches of psychology and an emerging recognition of their intimate connections with one another (see, for instance, Bruner, et al., 1957; Adams, et al., 1954; Hilgard, et al., 1952; and Brunswik, 1952).¹ Hegel's dialectic, perhaps, has at least one empirical referent in the evolution of scientific thought.

¹ See pages 211–214 for complete bibliographical data on text references throughout this paper.

Whether we, in political science, will be rewarded for our logomachic efforts by the rich prize of intellectual discovery is a matter of conjecture. So far, the chief effect of the debate has been a heightened sensitivity to the importance of theory. The new breed of political scientists is fond of accusing the old-liners of insufficient sophistication in "theory," while the old-liners accuse the "behaviorists" of a seemingly similar crime: that of neglecting significant theoretical questions for the sake of empiricism and of neatness in research technology. As a result, "theory" is now to the political scientist what motherhood is to the politician: you cannot go wrong if you are for it.

Such apparent unanimity amidst contentions is not necessarily heartening; it rather makes one suspect the presence of some confusion. In the first section of this essay, I shall consider two meanings of the term theory which may account for the confusion. The second section will focus on the possibilities of so-called "system" theory in political science and introduce, as illustrations, three sketches of possible theoretical models. In the last section, I will briefly survey some of the more recent research procedures which might help in translating theoretical ideas into practical research.

THEORY IN POLITICAL SCIENCE²

Political scientists use the term "theory" with a variety of meanings. If we restrict our discussion to descriptive theory—leaving aside the history of doctrine as well as political philosophy—at least two kinds of theory are still left. I will distinguish them by the adjectives "phenomenal" and "systematic." In phenomenal theory, the facts of experience are at the focus of attention: we wish to explain the weather, the reasons for differences in the behavior of individuals, or the emergence of dictatorships. In systematic theory, attention is focused upon a logical system by the aid of which we explain phenomena carefully and purposefully abstracted from the world of experience: for the sake of theoretical neatness and clarity we treat of such abstract entities and processes as mass-points, perfect gases, or choices under certainty. Thus, for the two types of theory, our world is organized according to different principles: for phenomenal theory, we allow the physiology of our senses and the culture-bound categories of our understanding to define the range and context of relevant observations; whereas for systematic theory, we deliberately select and relate to each other those aspects of events which lend themselves best to logically coherent explanations.

The propositions belonging to a given systematic theory form a logic

²I am indebted to Professor Richard Rudner, of the Philosophy Department at Michigan State University, for advice on this section. All errors are mine.

system; most of the propositions, called "theorems," can be deduced from a smaller set, called "axioms." Phenomenal theories lack such internal logical consistency; their propositions often belong to several universes of discourse or are in other ways logically independent. This distinction—I shall argue presently—has far-reaching consequences for research methodology. But first to illustrate: Duverger's *Political Parties* (1954) contains phenomenal theory. It brings together a variety of propositions containing the term "party," irrespective of the universes of discourse from which they are drawn, e.g., those dealing with leadership characteristics, properties of organizations, effects of electoral systems, etc. Being composed of such disparate parts, the theory cannot possibly be tested for internal consistency. Downs' *Economic Theory of Democracy* (1957), on the other hand, is a systematic theory. Each concept is explicitly defined and the demonstration that the propositions comprising the system are logically consistent occupies the major part of the book.

I shall now discuss the verifiability of the two types of theories, and I shall argue that the truth of phenomenal theories depends on the truth of related systematic theories. From this it will follow that no discipline can be built upon phenomenal theories alone, and that close attention to systematic theory is, in underdeveloped areas of knowledge, the first order of business.

Verification of theories consists in at least two operations:

1. We test the empirical truth of propositions composing the theory.
2. We test the logical compatibility of such propositions.

The second test is necessary because evidence gained from the first test nearly always leaves us in a state of considerable uncertainty.

Strictly speaking, no proposition is ever verified by any empirical test we may apply. We can say only that it has not been falsified. Suppose we wished to test the proposition that, for all pendulums in motion, the direction of swing remains constant. Experience seems to support this generalization. In order to test it we now must develop hypotheses about the conditions under which the direction of swing might change. We guess that the length and mode of suspension of the pendulum might have something to do with the matter; we keep varying these and observe, under these changing conditions, the direction of the pendulum swing with respect to a line drawn on the ground. So long as the points of our pendulums faithfully follow the line on the ground, we keep ruling out various specific hypotheses about the conditions under which the direction of swing might change. Thus we continue to believe the generalization about the constancy in the direction of pendulum swings to be true. Yet we have no direct confirmation.

Suppose now we use, in the course of experimentation, a particularly long pendulum suspended in such a way as to minimize friction; we find now that the swing of the pendulum changes in a clockwise direction. (I should add that this experiment is being conducted in the United States.) The generalization which we set out to test is now falsified. But immediately, we are lead to formulate some new proposition about the direction of pendulums, for instance: "Pendulums, if suspended from a low-friction joint, swing in a plane which rotates clockwise with respect to the surface of the earth." We now set out to test various hypotheses which might falsify this proposition, and finding none that can be supported by observation, we gain more and more confidence in the truth of the "clockwise" proposition. This confidence lasts until, one day, we receive a letter from a colleague in Australia who reports that all his pendulums change their direction of swing, too, but in a counter-clockwise direction. Conceivably, we might receive another communication from a colleague in Ecuador who informs us that his pendulums do not change direction at all. We must now look for a new generalization which would accommodate all the known data, one of the possibilities being the rotation of the earth.

This last proposition—that the earth rotates about its axis—is no more subject to *direct* proof than were any of the propositions previously considered. All we can say is that we know of no case in which a hypothesis inconsistent with this proposition failed to be falsified. We thus have great confidence in the proposition asserting that the earth rotates, but we do not entirely discount the possibility of its being false.

This "round about method" of gaining support for generalizations is quite general. It applies whenever the empirical facts to which a generalization purportedly applies are not available for direct and exhaustive inspection, either because the number of instances is too large or because our sensory apparatus is not equipped to make some of the relevant observations. Such generalizations are quite common, and they are held in particularly high esteem because they alone enable us to think rationally about situations which we have not previously observed.

Assume now that we have two generalizations, logically inconsistent with one another, and that we wish to make a choice between them, declaring one acceptable and the other unacceptable. The tests which we have conducted do not yield any criterion for making this decision; they only tell that neither of the generalizations has so far been infirmed. Thus, judged by empirical tests only, both generalizations have equal truth value. Clearly, we now need further criteria for making

the decision. One such criterion is that of logical consistency with the rest of accepted knowledge. In practice, all other things being equal, we accept generalizations which fit logically into the existing body of theory, while rejecting those inconsistent with it.

If it were not for this criterion of consistency, human knowledge would be of little use. It would be a collection of unconnected propositions giving no directions for action or for further research. Such a disjointed body of knowledge would scarcely fulfill the purposes for which we intend it: reducing our perplexities and increasing our ability to make rational choices. The application of the criterion of consistency makes for an essentially conservative policy in the management of human knowledge: we entertain new ideas only if forced to do so by empirical facts inconsistent with existing theory or if such new ideas would increase the logical consistency of existing theory.

At the same time, the discovery of logical inconsistencies often serves as a potent stimulus for important intellectual advances. The theory of light is now a classical example of the internal stress within a discipline forced for a time to adopt as true two logically inconsistent theories—in this case the corpuscular and the wave theory. The resulting logical difficulties led to the formulation of the photon theory, which is now regarded as most satisfactory. (For a discussion of this issue, see Frank (1957), pp. 189–206.) Cumulative growth in a field of knowledge, it may well be argued, depends to a large extent on the discovery and resolution of logical contradictions.

The requirement of logical consistency thus has at least two functions: it serves as a criterion in deciding upon the acceptability of propositions and, by revealing inconsistencies, it stimulates new theoretical formulations. Accordingly, theories, to be testable, must be in such a form that inconsistencies can be discovered. Phenomenal theories, as I have defined them, are not in this form. Each of the propositions contained in a phenomenal theory must be tested separately by establishing its logical consistency with those *systematic* theories from which they can be logically derived. Phenomenal theories thus depend, for their acceptability, upon the verification of systematic theories. *Quod erat demonstrandum.*

Let us now apply the conclusions thus far reached to the area of political and administrative research. Clearly, knowledge in this field has not been, thus far, systematized to the point that internal logical checks are possible. And so long as we have no systematic theories in this field of knowledge, propositions dealing with political phenomena will have to be referred, for tests of their logical consistency, to other, more systematic theories.

The question whether a systematic political theory is within the realm of possibilities is not directly relevant to the present discussion. In the conduct of research, we can be guided only by the state of knowledge as it exists now, not by a state expected to exist at some later, yet quite undetermined, date. Still, this question has some bearing on the allocation of our research efforts. If we believed that systematization is just around the corner, we might be well advised to bend our best efforts toward its early achievement. If, on the other hand, the development of systematic theory appears either distant in time or uncertain of realization, we should prefer to concentrate upon the development of phenomenal theory. I should add that the existence of an academic field of study, administered by a separate department, does not imply the practical feasibility of systematization. Most university departments will perhaps never become managers of logically coherent chunks of human knowledge, and they will forever have to rely on their brethren in other departments for the development of the logical systems needed to verify their propositions. Nothing compels us to think that political science departments and bureaus of government or public administration must some day become proprietors of an independent scientific domain.

At the present time, the prospects for systematic political theory do not appear promising. David Easton, who in recent years has called most forcefully for the study of the "political system," has supplied only a definition of the set of phenomena to be studied (all processes leading to the "authoritative allocation of values" in a society) and has proposed a set of terms which appear to designate properties of an (unknown) set of elements, such as "regime," "demands," "support," and "policies." But there is no attempt to define the elements of the system, to describe a particular state of the system at some time, or to formulate laws showing how the state of the system at one time can be inferred from the state of the system at another time. The absence of all needed specifications reflects the inchoate condition of systematic theory in politics.

In a special area of political science, that of formal organization, James March and Herbert Simon (1959) have recently gathered up as much systematic theorizing as they were able to uncover. In only a small portion of their volume were they able to present theoretical statements dealing with organizations as independent empirical systems. Most of the generalizations and theoretical ideas contained in their work had to be gathered from psychology, economics and sociology; March and Simon, despite their strong bias in favor of theoretical model building, would be among the first to argue that the study of

formal organization will, for some time to come, be greatly in need of systematic knowledge gained elsewhere.

Under these circumstances I believe it unwise to gamble on the early emergence of systematic theory in political science and its sub-disciplines. In formulating generalizations, political researchers will have to look to other, more highly systematized areas of the social sciences (not necessarily organized disciplines) for support.

The research strategy which I advocate should not be confused with methodological "reductionism." The reductionist position assumes a hierarchy of intellectual disciplines; processes described by the "higher" disciplines can be expressed in terms of the "lower" ones, but the obverse (description of lower in terms of higher processes) is not possible. Thus, von Neurath believed that the languages of all sciences should ultimately be reduced into physical terms. Whether it is useful to postulate such a hierarchy of sciences and whether the proposed reduction is either feasible or desirable need not detain us at this point. My requirement of "consistency" is much less stringent at any rate. It is one thing to call for total homogenization of all scientific language, and it is quite another to require that generalizations in a given field of inquiry be logically compatible with such logical structures as can be brought to bear upon it.

BEHAVIOR SYSTEMS

I shall now sketch three types of processes which might lend themselves to systematic description. The ideas upon which I rely have been developed chiefly in economics and psychology. But the systematic theories to which I think political processes should be referred have not yet been fully developed. Possibly, students of political and administrative life may have to develop certain sideline specialties to help organize data and concepts in adjoining fields so that their logical bearing upon political and administrative phenomena may be more easily determined.

Theoretical models never emerge full-blown from the heads of their creators. In their nymphal state, they are rather formless images of the logical structures they are to become. Such images bear to future theories the same relation as the painter's first sketch to the final painting: although vague in outline and gingerly drawn, they nevertheless exhibit a basic over-all design showing the chief dimensions and relationships. Inasmuch as such images are imprecise, arguments can easily arise over their potential worth; this in part explains the everlasting disputes of the methodologists.

As labels for the three processes, I have chosen the terms "behavior

trimorphism," "segregation and integration processes," and "selection processes." The three types of processes are thought to explain social and political events on different "levels": they are related to one another in the same manner as micro-economics is related to macro-economics, and as the theory of economic fluctuations is related to the theory of economic change. Under the heading of "behavior trimorphism" I discuss three basic mechanisms of individual and social choice which appear to control social events; this suggests a molecular analysis of choice processes underlying large-scale events. In drawing the outlines of a "segregation and integration" model, I deal with certain large-scale social effects of the choice mechanisms under varying conditions: the segregation and integration processes are thought to explain cyclical fluctuations as well as steady directional changes in social and political systems. The "selection processes," finally, are expected to account for "irregular" changes, i.e., changes which cannot be ascribed to any given system of action but are the combined effects of several systems.

Behavior Trimorphism

Approaches to the development of political models have thus far tended to follow the reasoning of utilitarianism and economic theory. In these systems of thought, avoidance of pain and attainment of pleasure are regarded as similar strivings, different only in their directions. In traditional and marginal economic theory, both pleasure and the avoidance of pain have utility; in making economic choices, the individual is believed to maximize utility by equalizing the marginal utilities of all goods and services he acquires. So long as we speak about pains and pleasures which have a market price, this does not seem unreasonable. People do ask themselves the question whether they should buy an additional amount of insurance or rather eat out more often—and they do seem to come to decisions about such matters. In the same fashion, members of city councils seem to be able to balance off increased amounts of police protection against the construction of new playgrounds, i.e., equalize the utilities of both at the margin.

All the cases we have mentioned are controlled by the well-known law of diminishing marginal utility. As the individual keeps acquiring increments of various commodities, their marginal utility decreases; if he is careful in managing his resources, the marginal utilities of all commodities will have declined to the same level at the end of the process, which occurs when the marginal utility of his remaining liquid assets equals that of any commodity increment. The belief that all marginal utilities diminish is based on a notion of satiation. As one

still his hunger, interest in food decreases; as one acquires more and more desk lamps, the desire for desk lamps goes down.

The applicability of this model to political behavior is implicitly assumed in much current thinking. Thus, Harold Lasswell draws up a list of eight "values" (i.e., "preferred events"), which includes both "wealth" and "well-being"; in the absence of any specific distinctions, we are led to believe that the processes of acquiring these values are in both cases quite similar. The "group" theorists, David Truman, Earl Latham, and others also have never attempted to distinguish between processes of group striving for positive goods and for protection from harm. Anthony Downs, in his *Economic Theory of Democracy* (1957), explicitly states that voters derive from government a "stream of utilities." However, he departs from the traditional utilitarian model when he ascribes to government a desire to maximize votes rather than utility. Clearly, votes guarantee the government's security in positions of control, and Downs seems to indicate that the law of diminishing marginal utility does not apply to the government's search for its own security.

Going beyond Downs, one might wonder whether the law holds for security measures generally. The observer of administrative behavior is often struck by the elaborate network of formal and informal rules employed to prevent the actual or suspected occurrence of bribery, embezzlement, and nepotism, as well as violations of status and specialty lines. The practices which the vernacular calls "red tape" constitute a complicated network of protective devices used to avoid illegal actions, violations of etiquette, and threats to the integrity of organizations. Is the acquisition of such protective devices subject to the law of diminishing marginal utility? The answer to this question is doubtful. In some administrative organizations there may be no point at which an additional increment in honesty becomes negligible and hence undesirable. Often, the object of security measures is not merely to maintain honest performance at a reasonably high level but to guard against any and all dishonesty which, if discovered, might lead to scandal and to political difficulties. In such circumstances, marginal utility can hardly be thought to decrease as security increases.

The same question might be raised with respect to the national defense effort. It is rather difficult to believe that the marginal utility of implements of war decreases as more such implements are acquired. (For simplicity's sake, implements of war are assumed to be homogeneous.) The reason for doubt is the same as in the previous example. There appears to be no point at which the acquisition of more security is not worthwhile. For the object of the defense effort is not just a

"reasonable" security level, but the avoidance of any possibility of defeat. Similarly, the law of diminishing marginal utility apparently did not apply to the Communist hunts of the late Senator McCarthy.

Thus, there may be commodities whose marginal utility does not decrease. Suppose, for instance, that a man was obsessed with the idea of security for himself and his family and that each incremental \$1,000 of insurance had to him the same utility as any other \$1,000 of insurance. What would be the consequence? In the first place, the range of commodities which this man might consider buying would be narrowed; he would exclude all commodities the smallest usable quantity of which has less utility than the fixed utility increment of \$1,000 of insurance. All other commodities would be purchased until their marginal utilities had become equal to that of \$1,000 worth of insurance. If, at this point, the marginal utility of the buyer's liquid assets were still larger than that of insurance, he would continue to purchase insurance.

The examples of administrative and national defense policy suggest that such processes are common in political life. More specifically, I think that the marginal utility of items acquired in response to anxiety does not decrease. If this were true any model of political and administrative life would have to take this fact into account. Some insight into this problem can be gained by reviewing certain findings of learning theory.

The question as to whether behavior under reward is controlled by the same laws as behavior under punishment has received the recurring attention of learning theorists. A few, such as Guthrie, have argued that escape from pain or injury is rewarding and that pain *avoiding* behavior (when no noxious stimuli have been applied during training) is in the nature of an anticipatory response which, in some unexplained way, is learned as a correlary of *escape* responses (Hilgard, 1956, pp. 60-62). Other authors, struck by the fact that responses learned under punishment are much slower to extinguish³ than rewarded responses, have expressed doubt about the symmetrical nature of the effects of reward and punishment. The earlier learning theorists, being devoted servants of scientific parsimony, attempted to explain *all* learning—of both goal-directed and avoidant behavior—by a single principle. For a while, two principles of learning vied for the allegiance of the scholars: the principles of contiguity and reinforcement. According to the principle of contiguity, a response is thought to become condi-

³"Extinction" is the technical term used by learning theorists to designate the diminution (in frequency or strength) of the conditioned response when the conditioned stimulus is presented without the unconditioned stimulus (i.e., in the absence of "reinforcement"). Resistance to extinction is considered a measure of habit strength.

tioned to any neutral stimulus, CS, within the organism's field of attention at approximately the time this response is elicited by noxious stimulus UnCS. The principle of reinforcement states that a response becomes conditioned to a stimulus CS if, during training, the response is rewarded. But the proponents of either principle were never entirely successful in explaining both goal-directed and avoidant behavior.

More recently, many learning theorists have, under the weight of evidence, resigned themselves to postulating two distinct learning mechanisms. In 1947, O. H. Mowrer (1950) suggested in a pathbreaking article entitled "On the Dual Nature of Learning" that avoidance responses are learned by association conditioning (or contiguity), while "operant" (Skinner, 1938, pp. 20ff.) responses are learned because their effects are reinforcing. In the case of avoidances, a neutral stimulus present at the time of noxious stimulation becomes a danger signal; whenever this stimulus is perceived, the organism will exhibit an avoidant response. In the case of operant responses, no stimulus is needed; any response will become reinforced if it reduces a drive.⁴ Mowrer believed that "conditioning," as narrowly conceived by him, accounted for emotional, i.e., visceral and vascular, responses, whereas effect learning accounted for behavioral responses, i.e., for activity of the skeletal musculature (Mowrer, 1950, p. 244).

In support of his position, Mowrer cited an extensive literature describing experiments with animals and humans. From this survey emerged two major differences between the two types of learning. Both are important for our purposes. *First*, noxious stimuli result in much faster acquisition of the conditioned response than does reinforcement in effect learning. *Secondly*, conditioned anxiety responses extinguish much more slowly than do conditioned operant responses.

In a dramatic experiment, Solomon and Wynne (1954) demonstrated that these two generalizations held particularly well in the case of *trauma*, which they generated in dogs by administering "subtetanizing" shocks. The avoidant response was invariably established after a very few—on occasion a single—trials. Furthermore, the response so established showed no sign of extinction after numerous trials and long periods of time. By careful training, it was possible to reduce the frequency and amplitude of the conditioned response, but in no case did the authors succeed in eliminating it completely.

These differences between behavior acquired by conditioning and by effect learning are often ascribed to physiological causes, such as

⁴ Mowrer has subsequently made radical changes in his two-factor theory, particularly with respect to the genesis of anxiety. These revisions need not concern us, since we are less interested in the theory offered to explain the two learning processes than we are in the nature of the processes themselves.

presumed differences in the functioning of the autonomic and the central nervous systems. Whatever the explanation, the two modes of functioning clearly have survival value for the organism. Skeletal activities, which serve the functions of food gathering, mating, etc., tend to disappear unless reinforced at least on occasion; clearly, their persistence without reinforcement would be highly inefficient. On the other hand, danger avoidant responses cannot possibly be reinforced so long as the organism persists in avoiding danger; it is clearly to the advantage of the organism for such responses to be maintained in the absence of actual exposure to danger. Indeed, laboratory experience seems to indicate that exposure to the noxious stimulus *after* the conditioned response has been established is not reinforcing but rather weakening; such exposure seems to "confuse" the organism by "telling" it that its conditioned response is perhaps not entirely appropriate for avoiding pain and injury.

The foregoing descriptions suggest rather obvious parallels in human behavior. We have known people who, after one serious car accident, were forever incapable of driving, and there is the proverbial child which once burned avoids the fire. Compulsive behavior in anxiety neuroses is the same phenomenon in more extreme form. On the other hand, a person who has learned that there is money to be made at the race track or female companionship to be found in San Francisco's International Settlement will abandon his search if it remains for long unrewarded. Note, however, that the race track and the Settlement may, under some conditions, become objects of a neurotic attachment born of anxiety; if so, the behavior is likely to persist.

In suggesting two behavior mechanisms (which reflect two kinds of learning), I have thus far disregarded the high level of symbolic functioning in human beings. But people are capable of rational calculation, at least insofar as motives, stimuli, and responses are conscious; the question whether there is also unconscious calculation must remain unexplored. Calculation involves the ability to make comparisons between "imagined" situations (conditions of drive, environmental configurations, effects of behavior) which are not directly evoked by the stimulus field at any given time but which are summoned up into consciousness by virtue of man's cognitive organization. Insofar as calculation controls behavior, the process appears to consist of assigning two sets of weights to all possible outcomes: probabilities and preferences. This activity is described by the game-theoretic concept of "utility." Under the definition of the game theorists, utility is a real-valued function defined over "lotteries," where each lottery is a set of

alternatives to which probabilities are assigned (Luce and Raiffa, 1958, Ch. 2).

I have thus defined three behavior mechanisms, which, for the sake of simplicity, I shall henceforth label "productive behavior," "avoidance behavior," and "calculated behavior." "Production" is taken to be motivated by primary drives and reinforced by its effects; "avoidance" is motivated by the secondary drive of anxiety and is conditioned to anxiety-arousing stimuli; "calculated behavior" results from comparisons of gains and losses to which probabilities are assigned. "Production" obeys the law of diminishing marginal utility, "avoidance" that of response perseverance under anxiety, and "calculation" that of game-theoretic utility maximization.⁵

It may have been noticed that this formulation coincides rather closely with Freud's theory of personality structure (1927; 1932; Munro, 1955, pp. 82-89). The id is regarded as the unconscious organization of primary (organic) drives; it operates according to the "pleasure principle." The superego is the repository of secondary (learned) drives and inhibits the id. The ego is the calculating mechanism whereby the reality principle is applied and id and superego drives are made to conclude a viable "compromise."⁶ Both ego and superego are in part conscious.

Various aspects of personality patterns are usually assumed to result from the relative predominance of each of the three "institutions" of personality. A strong ego means rational behavior. A weak ego means either anxiety, inhibition, guilt, and sometimes moralistic rigidity (if the superego is predominant), or audacity, impetuosity, absence of guilt feelings, and sometimes immorality (if the id is in control).

Many phenomena of government and politics may well be understood as manifestations of the three behavior mechanisms. Two broad categories of questions might be asked:

1. If we hold personality type constant, what effects are likely to

⁵ It will be noted that marginal and game-theoretic utility are different concepts and do not refer to the same behavior mechanisms. Marginal utility can be interpreted, in terms of learning theory, as either drive reduction or extinction of the orientation reflex (Konorski, 1948, pp. 78-79); it does not necessarily involve symbolic behavior. Game-theoretic utility, on the other hand, calls for a high level of symbolic activity.

⁶ Political scientists may find it stimulating to reflect on the similarity between this theory and the separation of powers, particularly in the latter's earlier forms. The legislative body tended to represent the productive, crudely acquisitive layers of the population and make demands for material improvement and freedom of action. The monarch represented the controlling elements whose task it was to maintain order, morality, and decorum; he had the veto power. The courts, of course, occupied themselves at resolving conflicts between these two powers in the light of reality; as Freud's ego, the judicial institution had no power of its own.

result in the political system from the interaction of the three behavior mechanisms?

2. Given a range of personality types, what will be the effects of their interaction?

The first set of questions asks, in effect, what behavior mechanisms will dominate the activities of any group of people, under conditions to be specified. We might hypothesize that, so long as the perceived rewards of cooperation are low, avoidance behavior will be predominant. It will be remembered that avoidance behavior does not extinguish, rather, it is triggered by anxiety. It is possible to raise any person's anxiety level considerably by mere symbolic presentations of conditioned stimuli, such as the words "cancer," "Communist," "fall-out," "inflation," and the like. Since such threatening cues are likely to occur frequently among any group of people communicating with one another directly or through the mass media, we hypothesize that *all human groups operate in such a way that anxieties will constantly be regenerated and avoidant behavior generated*. On the other hand, productive behavior extinguishes unless it is reinforced. It seems to us that there is no reason to suppose that social interaction is as such likely to be reinforcing; that is, a given behavior will be reinforced if rewarded irrespective of whether it occurs in a context of social interaction or not. It is only where group productivity—and hence the individual reward—is particularly high that increased reinforcement of productive behavior will counterbalance group-generated anxiety.

Now political groups and—even more so—the apparatus of government are largely nonproductive. Even in the relatively restricted area of productive governmental activity reinforcement of productive behavior is unlikely to occur; since government-produced goods usually are collective, the connection between behavior and reward cannot be easily perceived. This holds particularly for voters: the bearing of their voting choices on utility derived from government action is, at best, a matter of highly educated guesses, not of immediate experience. While the successes of small political groups can at times be reinforcing, government at large usually does not involve many individuals in productive behavior. At the same time, anxieties are constantly being generated and maintained. And since politicians and administrators are only rarely in a position of rewarding behavior directly (bribes are unfortunately forbidden), they are bound to resort to the systematic exploitation of anxieties.

All large scale political processes are therefore likely to be both products and sources of anxiety, as Hobbes saw so clearly some three hundred years ago. The typical forms of political behavior are all avoid-

ances. Besides withdrawal (on the part of the least-involved citizen), there is the constantly reiterated affirmation and testing of political loyalty, partisan as well as national, which is either a "hiding in the herd" reaction or a posture of defense taken in expectation of attack and destruction. There are the protective systems of status and bureaucratic position carefully worked out in communities and administration. There is everlastingly the readiness to oppose change, even if deemed desirable, because of the unknown dangers it might harbor. And there is the reliance on protective authority and, in extreme cases, on authoritarianism.

Under these circumstances, it is hardly possible to conceive of political and administrative life as equilibrium processes, in the sense in which such writers as Truman (1949) and Easton (1953) speak of them. Their models rest on vague analogies between economic and political behavior. Lasswell is guilty of the same mistake of analogizing when he accords identical treatment to such "values" as "wealth" on the one hand and "respect" and "well-being" on the other (Lasswell and Kaplan, 1950, pp. 161 ff). His error is all the more astonishing as he is well aware of the genesis and impact of personality patterns in the "political arena."

What, indeed, will be the effect of a predominantly anxious group assuming positions of high visibility in a given society? Or of a particularly unscrupulous group of leaders? Under what conditions will the ones or the others be tolerated, and what conditions make for rational leadership? These are some of the questions suggested by the differences in personality structure—and they do not point very directly to an equilibrium model, at least not to one which conceives of the political process as a balancing of group demands.

To raise such questions and even to make general assertions about the place of avoidance behavior in social and political life is, of course, not doing a great deal. General propositions of the Hobbesian variety express nothing more than a methodological viewpoint—they are not theories of political or administrative life. We shall have a beginning of theory when we can specify the conditions or events which cause anxiety to increase or decrease among particular individuals or groups of people; or when we know under what specific conditions productive behavior will be reinforced or extinguished in social and political situations; or when we have learned something in detail about the interaction between various types of leaderships and publics. More specifically, we shall make progress toward theory if we are able to explain levels of defense expenditures—for instance—as a function of anxieties generated among various groups of people by threatening cues; or if, in

similar terms, we succeed in explaining strength of party affiliations and party ideologies; or if we can specify the conditions under which administrative changes can be made in the face of protective practices; or if we succeed in explaining why state legislators appear to be so much more resistant to change than are congressmen.

Such questions need not be answered in their concrete historical settings. Indeed, the needed information often is not likely to be accessible and, if accessible, to be too confusing for systematic analysis. We may thus be forced to resort to methods which have proved so successful in other disciplines, for instance, those of experimentation.

Segregation and Integration Processes

The behavior mechanisms may yield insights into certain aspects of a political or administrative system; they may even enable us to predict its possible alterations. But they are not likely to help us assess the stability of any system or to yield concise descriptions of fluctuations which a political or administrative system is likely to undergo. This is so because the mechanisms are attributes of behaving entities—individuals and relatively small groups; they are not characteristics of the system as a whole.

A general understanding of fluctuations in policies and, above all, in constitutional practices is greatly needed. When we are called upon to submit proposals for administrative reorganization or for community development, we must have some basis for predicting the course of constitutional development which our suggestions might entail. Clearly, we place a high value on such things as opportunity for participation and guarantees of rich and rewarding lives for the citizens. The question often arises whether one should try so to design plans for governmental or community organization as to satisfy the desires of as many people as possible, or whether precedence should be given to some, presumably more ethical, desires. And indeed, what are these desires? The man in the field often finds it difficult to find out what people want—if they want anything specific at all.

Our preoccupation with desires, wants, preferences and the like is another instance of the manner in which economic analogies have colored our thinking. At bottom, we wish to build and maintain systems which will enable people to pursue their interests and seek their satisfactions with some reasonable chance of success. Political and administrative systems are not directly providers of gratifications; they are merely regulatory systems which open and close the avenues toward rewarding experiences. The availability of such avenues depends to a large extent on the strength of boundaries enclosing human groups,

such as social classes, castes, religious communities, occupations, geographical political entities, voluntary associations, and primary groups; in part they also depend upon the permeability of groupings, i.e., the amount of overlap in membership, which can facilitate social mobility.

Georg Simmel (1922; translated by Bendix, 1955) recognized the great importance of “web of group-affiliations” for individual self-determination (pp. 140ff). He also realized the close correspondence between the cognitive systems of individuals and the system of social segregation and integration; the constructs which people use to organize their experience resemble rather closely the actual social groupings they observe. In the present section, I will deal with the group structure of society, and in the next section with the relationships between the articulation of a social system and the cognitive systems of participants.

To begin with, I shall proceed somewhat formally. Let an individual be defined as some entity (e.g., person, firm, family) with the following properties: he can be a member of one or several given groups, or not a member; he can join or leave groups; and he can engage in interaction with other individuals either within a given group or without it. A group is a collection of individuals with the following properties: it has boundaries that keep some individuals inside and others outside the group (called “members” and “nonmembers”); it can be attractive to members and/or nonmembers or it can repel them. Society is the collection of all individuals who are in some sense interdependent (above some minimal level), whether or not they are members of groups.

I now define “cohesion” as the amount of attraction a group has for its members and I define “rejection” as the strength of the boundaries a group maintains with respect to nonmembers; cohesion and rejection are both factors in “segregation.” Finally I define “integration” as the frequency and intensity of interactions between individuals both outside and across group boundaries.

Under these conditions, any society can find itself in one of the following four states:

		Integration	
		High	Low
Average Segregation	High	Pluralism	Caste or class society
	Low	Mass society	Disintegration

The terms in the cells of our diagram indicate roughly the kinds of society I think will be produced by combinations of the segregation and

integration variables. It will be intuitively clear that the states of "class or caste society" and of "mass society" are fairly stable, whereas the other two states are relatively unstable. Since pluralism interests us most, let us briefly consider under what conditions this state is likely to be maintained.

Assume that, for any reason, some important groups in society become more cohesive. If other groups respond to this occurrence by becoming more cohesive themselves, the system is likely to move in the direction of a segregated society with strong distinctions of status and situs. If, on the other hand, a sufficiently large number of individuals leave their groups and if other members of these groups respond by doing the same, the society is likely to be atomized (i.e., become a mass democracy or an authoritarian system). In order to maintain the system in the state of pluralism, it is therefore necessary that there be countervailing forces: in general, swings in the direction of segregation must generate forces for integration, while swings toward integration must trigger segregation movements. Limits must, of course, be assigned. Many groups can lose members and even disappear before the pluralistic character of the society is in serious danger, and, conversely, a certain amount of group consolidation must still be consistent with the maintenance of the pluralistic system.

The conditions contributing to the maintenance of a pluralistic system might well be an object of serious theoretical study. Simmel believed and Leaderer agreed that the system was maintained in virtue of the personality structures which are frequent in a pluralistic society; multiple memberships and "cross-pressures" (Simmel invented the term which has since become so popular among students of voting behavior, see p. 146) cause individuals to resist excesses of segregation and integration; moreover, each individual's involvement with many different kinds of people brings about a kind of social morality which rejects both discrimination and depersonalization.

Whether this is so is a matter of conjecture. Clearly, under the impact of events a pluralistic system will forever be oscillating between segregation and atomization. The amplitude of the swings and, indeed, the reversibility of each swing will depend on the nature of the events and upon the characteristics of the process whereby countervailing forces are generated.

To be sure, it will prove extremely difficult to study such phenomena in the field. It might even be foolhardy to approach this problem experimentally before a theoretical model has been worked out. Such a model might well take the form of a stochastic process (see Bush and Mosteller, 1955; Kemeny *et al.*, 1959).

It might consist in a matrix containing the probabilities for members of all groups to move into other groups; such probabilities might be assigned on the basis of empirical social mobility indices. To move the system to its next state, an operator must be applied to transform the probabilities. What might be the nature of such an operator? Ideally, this operator should be a function of the differences between the gain-or-loss expectations of groups, i.e., of the differences between probabilities. The reason for this definition of the operator is as follows: groups that are likely to lose or gain members will exhibit certain signs of anxiety or of satisfaction that will make them more or less cohesive or attractive. Thus, social mobility can be seen as a function of anxiety and satisfaction levels. Furthermore, the model might be so constructed so as to take threatening or rewarding events into account; i.e., such events should change the probabilities of single and multiple group memberships in opposite directions.

Selection Processes

The kinds of mechanisms so far suggested are expected to illumine the *normal* functioning of a political or administrative system. To be sure, even these processes can conceivably "get out of hand" and thus produce lasting changes. I believe, however, that the system itself is more likely to be altered by changes in the selection processes whereby new social groups and new leaderships are recruited. Changes, then, are the consequences of discrepancies between coordinated systems, e.g., the personality systems of leaders and the role system which is embedded in the social matrix.

I have already mentioned the similarities which Simmel saw between the network of human concepts and the "web" of group affiliations. Such correspondence does not seem startling; after all, people form their concepts through experience in the social world, and the language they learn is likely also to reflect the categories of people into which a given society is customarily divided. Moreover, to the extent that man is the creator of social divisions and institutions, these are likely to be the projections upon the screen of reality of his habitual patterns of thought (see Whorf, 1956, particularly pp. 134-159).

A growing awareness of this correspondence has impelled recent researchers to pay renewed attention to cognitive organization. While Whorf emphasized the effects of grammatical and syntactic forms upon thought and behavior, modern investigators have paid particular attention to man's concept system. A landmark in this effort is Bruner's careful study of concept attainment (1956), i.e., of the strategies used by

people in ordering their experience. In the course of his investigation, Bruner developed an eminently useful typology of concepts.

While Bruner feels that detailed studies of the conceptualization process *qua* process should be given first priority (see his critique of Festinger's "dissonance" theory in Bruner, 1957, pp. 151-156), other authors have fastened attention upon both the social and personality correlates of cognitive organization. Such researches have yielded initial evidence for the proposition that cognitive organization might well be taken as an indicator of many other personality characteristics, particularly those which are relevant for social and political analysis.

George A. Kelly (1955) has developed an elaborate theory of personality disorders based entirely upon cognitive organization. Central to his study is the overlap between classificatory concepts and the resulting "permeability" of the construct system; clearly, there is a formal resemblance between the construct system so defined and the "web" of group affiliations. Milton Rokeach (1954, 1956a, 1956b, 1960), taking his departure from the California studies of the authoritarian personality, has conducted a connected series of investigations into dogmatism. Political, religious, and—indeed—scientific-academic dogma are shown to consist in the existence of strong boundaries between belief and disbelief systems, in the maintenance of contradictions within the belief system, and in a drastic narrowing of the cognitive field due to discounting and denial of experience. An experimental study by Easterbrook (1959) makes a similar point by showing that the number of environmental cues which the organism will utilize decreases with rising emotion.

A few researches have shown—as Rokeach (1954) discovered early in his work—that conceptual structures bear relationships to such social variables as class and status and primary-group membership. Schatzman and Strauss (1955) divided participants in a disaster into groups by education and income; they found definite differences in perspectives, imagery, use of classifying and generalizing terms, style, and cognitive organization. Runkel (1956) demonstrated that similarities between two cognitive fields increase the efficacy of communication between them.

In view of these recent advances, it does not seem farfetched to argue that intensive analyses of the relationships between cognitive and social structures might throw much light upon the manner in which people are selected into social positions or else select social situations in which, given their conceptual apparatus, they are capable of functioning. I argue that there must be substantial congruence between social organization and cognitive structure for any system to persist over time. Thus,

an integrationist will feel uncomfortable in a segregated society; either his conceptual system or—if he is one of many or a man in a key position—the social structure will have to yield. Similarly, an “operator” will feel constrained in a tightly organized bureaucracy; either he changes his own conceptions or he subverts the system.

Our questions concern, of course, the conditions under which either of the two change processes is likely to take place. Do status and situs measurably affect the chance that a cognitive system will have effects upon the group structure of an administrative organization, a community, a society? Does formal position? In other words, is the problem of social and political change chiefly a staffing problem? Are there large-scale changes in the cognitive structures of groups, due perhaps to culture contact, which might affect the socio-political table of organization? Or do cognitive structures tend to conform to the system of social articulation in which they must exist. It is clear that none of these questions is likely to deserve a clear “yes” or “no.” Let us suggest once more that the permeability of boundaries, whether cognitive or socio-political, is likely to be a function of levels of anxiety and of satisfaction present throughout a given social system or at particular points within it.

PROCEDURES

In making suggestions for theory, we have implicitly assumed the possibility of certain types of data analysis. These will now retain our attention.

Three types of data-processing methods are clearly called for:

1. We need ways for ordering preferences and for comparing preference orderings.
2. We must be able to handle large batches of variables and to constitute types.
3. We must be able to identify configurational patterns and to define indices describing them.

Our greatest needs thus are for methods of *scaling*, of *multivariate analysis*, and of *configurational analysis*.

Scaling

Scaling methods nowadays enter nearly every phase of research activity. Usually it is necessary to know something about the order in which observed phenomena should be arranged before correlational analysis can proceed. If, in some research setting, we need to ascertain levels of anxiety or of satisfaction, some scaling methods would surely have to be applied.

In recent years, scaling methods that borrow their procedures from psycho-physical research and are aimed at the construction of interval scales have generally fallen into disfavor; they were criticized on the ground that they often were not unidimensional. Scales which achieve an ordering of both items and subjects have largely superseded the interval scales. Since Guttman's statement of the basic theory of cumulative scaling (1950), many refinements have been introduced into its theory and practice. A careful survey of all scaling methods will be found in a volume by Torgerson (1958), while specific problems of Guttman scaling are treated in Riley (1954). For an excellent theoretical statement of scaling theory see also Coombs' article in Festinger and Katz (1953).

The greatest difficulty in scaling still remaining is that of evaluating the extent to which the scale order is different from a chance order. The sampling distribution of the statistic which is used to measure unidimensionality, the Coefficient of Reproducibility, is unknown and appears highly unstable. Various other methods for determining departure from chance linearity have recently been explored (e.g., Sagi, 1959; Green, 1956; White, 1957; Goodman, 1959).

Approaches have also been made toward simultaneous scaling along several dimensions (see Guttman in Lazarsfeld, 1954, pp. 258-348; Torgerson, 1958, pp. 247-297; Torgerson, 1952; Messick, 1956). None of these methods has thus far had wide application. Siegel (see Siegel, 1956, and Hurst and Siegel, 1956) has made promising efforts in the direction of obtaining ordered metric scales from paired comparisons (see also Fagot, 1959). It is claimed that such scales have a special application in the measurement of utilities and that the distances between points on the scales are meaningful.

Multivariate Analysis

If we are to make comparisons between organizations, communities, or individuals, we are likely to be faced with large sets of indices. Usually, we are not so much interested in relating one particular measurement to another as we are in finding similar index patterns. If our data are quantitative, a first approach is that of comparing profiles of measurements. A variety of methods for the computation of profile similarity have been developed (e.g., Webster, 1952).

A frequently used method for processing quantitative data based on multiple measurement is factor analysis. The method reduces a large number of variables to a small number by using the information contained in a correlation matrix relating all variables to one another. The end product of the analysis is a small number of constructed variables,

called factors, which correlate with the original variables. For decades, factor analysis has been the exclusive domain of experts; the mathematical sophistication needed and the inordinate amount of labor often required made it prohibitive to the ordinary researcher in social science. Recently these difficulties have been largely removed. We have now good introductory descriptions of the method written for people of moderate statistical means (Fruchter, 1954; Adcock, 1954; Cattell, 1952). Moreover, the presence of high-speed computers on most larger campuses and the existence of "canned" programs has taken most of the work out of factor analysis.

Factor analysis was developed for the handling of data measured by an interval scale. The technique uses the Pearsonian correlation coefficient; it is subject to all of the restrictions applying to the latter. Difficulty arises in deciding whether serious errors are likely to result, in a given case, from applying the method to nonmetric variables and in the absence of normal distributions. A nonmetric factor analysis has, however, been proposed by Coombs and Kao (1955). A large variety of systems of cluster analysis, pattern analysis, and the like has sprung up in recent years (see Fruchter, 1954, for a lengthy bibliography, and Gaier, 1953).

Lazarsfeld's latent structure analysis (1950) is based on a generalized model of which factor analysis and scaling are special cases. This system will handle both quantitative and nonquantitative data. A description of the technique which will make it accessible to the work-a-day social scientist is not yet available.

A very simple method for handling both quantitative and qualitative multiple measurements was recently developed by McQuitty. Two versions of the system exist under the names of linkage analysis (1957) and hierarchical syndrome analysis (1959). Both methods aim at the establishment of types, the difference being that the types constituted by the first method are discrete whereas those constituted by the second method are overlapping and hierarchically arranged (according to strength of association). Analyses of the same sets of data by McQuitty's method and by factor analysis have yielded very similar results. The method recommends itself by its great simplicity, since it calls for nearly no calculations.

An ingenious method for collecting data that will be susceptible to easy factor analysis is Stephenson's Q-technique. This method will work only if the number of individuals to be studied is small, but it yields rich data. While Stephenson has employed the technique chiefly in clinical research, it will lend itself to various other uses. (For further

details see Mowrer (1953), Stern (1956), Block (1956), Jackson *et al.*, 1959.)

A great advance in our understanding of cognitive structure was made recently as a result of Osgood's invention of the semantic differential (1958). The technique consists in having individuals rate each concept of interest to the researcher along a fairly large number of scales defined by pairs of adjectives which are polar opposites. The scales are then factored, and the factors constitute a reference system which defines a "semantic space"; each concept can be located in this space. Osgood has adopted a number of scales whose factor loadings (correlations with the reference variables) are known. The three major reference variables found by Osgood are the evaluative, potency, and activity dimensions. The use of different scales with specialized populations might of course yield other factors. Development of additional scales, suitable for political and administrative investigations, would be highly desirable.

Configurational Analysis

The most venerable of the techniques of configurational analysis is no doubt Moreno's sociometric method (for descriptions see: Moreno, 1951, 1955; Borgatta, 1954; for a classification of sociometric literature, Nehnevajsa, 1955). Its status is so high that it rates a journal all of its own. As originally conceived, the sociogram was a simple graphic device in which people were represented by dots, with arrows drawn to show lines of attraction between them. This exhibited degrees of popularity and isolation of individuals and gave an impression of the overall group pattern. Since then, numerous indices of group characteristics have been developed, new applications found and refinements introduced (e.g., Criswell, 1955; Proctor and Loomis in Jahoda, 1951; Edwards, 1949; Torrance, 1955; Borgatta, 1954; Nehnevajsa, 1955). The technique is perfectly general and can be used to describe any system in which a set of relations is defined over a set of points. A more general statement of graph theory, applying to various types of networks, will be found in Harary and Norman (1953). Katz and Olkin (1952) developed probability methods for assessing the likelihood of given configurations.

While sociometric measurement describes a group at a particular point in time, interaction process analysis (Bales, 1950) takes account of the flow of information within the group and of the types of interaction which occur. This technique requires that the group studies (e.g., a committee, conference, etc.) be observed in action. The tech-

nique would appear to be particularly adaptable to studies of administration. (For a fuller statement of methods of group measurement see Hemphill, 1956. Group variables are discussed by Borgatta and Cottrell, 1955, and by the same authors with Meyer, 1956.)

A configurational method of interest to the community researcher might be that developed by Thrall and Angell (1954), the purpose of which is to assess the spread of group influence in the community. Using voluntary organizations for index construction purposes, the authors develop a detailed mathematical model of community influence.

Since researchers might be interested in relating the structure of communities or organizations to cognitive structures, they might want to use a technique developed by Kelly (1955) to describe individual construct systems. Indeed, it would be highly desirable if techniques could be evolved which would yield comparable descriptions of social and of cognitive patterns.

MODERN STATISTICAL TECHNIQUES

Many of the techniques mentioned in the preceding sections of this paper are rather uncomplicated; yet, they have thus far been rarely applied by political scientists. Among the members of the profession, there persists a tendency to shy away from rigorous methods of data-collection and processing. In the past, there existed at least one good reason for such reluctance: only relatively few methods were available for the processing of nonquantitative data. The political scientist felt that his data, usually, did not come in quantitative form; and where quantification was attempted, it often seemed contrived and led to the suspicion that conclusions drawn from such data might be quite artificial and untrustworthy. Being unaccustomed to the reading of methodological literature, he then did not realize that times had changed and that techniques requiring no more than ordinal measurement or qualitative classification had now become bountiful.

Until very recently, the nonparametric statistical techniques were still hard to come by, their descriptions being scattered in a great variety of journals and obscure monographs. Comprehensive descriptions of these techniques are now available (Siegel, 1956; Fraser, 1957; Mosteller and Bush in Lindzey, 1954; Chapin, 1955). Rapid developments have taken place in the area of rank order correlation which is of particular interest for the comparison of individual preferences (Kendall, 1948; Jardine, 1958; Haberman, 1955; Cureton, 1956.) Thus, the former dearth of statistical techniques applicable to political data has now been remedied.

CONCLUSION: RESEARCH DEVELOPMENT AND THE FUNCTIONS OF THE BUREAUS

The preceding paragraphs contain an implicit suggestion that the bureaus of governmental and administrative research in American universities should devote themselves to the development of abstract theory and formal research procedures. This may seem a rather radical proposal implying an abrupt change in the orientation of the bureaus. I suggest that it would, indeed, mean a change in current practices, but perhaps not so radical a departure from the ethic and the objectives which have in the past informed bureau activities.

In former days, the bureaus had four major assets, which accounted for their strength:

1. A central theory.
2. A strong ethical impetus combined with an action orientation.
3. An empirical attitude toward political events.
4. An ability to enlist the interests and cooperation of several disciplines.

Historical erosion has deprived the bureaus of most of these advantages. Let us briefly consider these four points:

Theory

Historically, the theory of the bureaus was normative in character and puritanical in content. It concerned itself chiefly with the elimination of malfeasance and corruption in government and with the individual's rights to freedom and participation. The theory stated that abuses could be eliminated by the adoption of adequate practices of management and administrative supervision and by guarantees of sufficient popular controls.

While this theory still appeals to the common sense of many, it has lost much of its power of conviction. The reasons are twofold:

1. Shifts in our ways of thinking have led to a changed conception of government; while once government was conceived as separate from and opposed to the "interests," we have now come to regard interest-participation as part of the governmental process.
2. We have lost faith in the effectiveness of purely legal and organizational arrangements as tools for obtaining far-reaching changes.

The weakness of the older theory lay not merely in its unrealistic emphasis upon purely formal-institutional change. The theory also suffered from—and ultimately became discredited because of—over-ambitious claims. The notion that the extremely complex processes of government could be the subjects of rigorous theories was easily deflated

whenever the results of reform did not correspond to expectation. This debacle resulted from the erroneous assumption that any set of phenomena, once it was identified as socially important, should and could become an object of independent, *systematic* theory. When such theory failed to materialize—or, if formulated, to be confirmed—discouragement took the place of enthusiasm.

Rigorous theory can deal only with rather special phenomena of an experimental or otherwise contrived nature—not with the fullness of human experience. Once this is recognized and once the small likelihood of systematic theory in our field is admitted, the bureaus will have to turn to existing systematic knowledge in the social sciences and make their work consonant with it. Thus, research work in the political and administrative field involves chiefly the utilization and elaboration of systematic theories developed elsewhere, not the development of *ad hoc* theories. Further, bureaus must learn to *recognize* the real-life situations in which general principles of behavior, gleaned from the social sciences, are likely to apply. This means that research in our area is to a great extent *diagnostic*.

Ethical Impetus

The reformist convictions of the earlier era put the bureaus in a position of teachers and guides with respect to the public and the officialdom. Bureaus believed that they had—and were believed by many to have—the “answers” to many social and political problems.

Two reasons help to explain why this ethical drive has disappeared from the governmental research movement:

1. The audiences for political evangelism have shrunk, as have the audiences for other movements of political salvation.

2. As the doctrinal core of the movement has been eroded in practical testing, the inner conviction of the reformers has tended to suffer.

As a consequence, from being teachers the bureaus have become servants of their clients. While client-orientation was once a means for changing—and bettering—the political world, it has now become a source of political constraints. In many instances, bureaus have become prisoners of their clienteles, with attendant loss of status among other groups of the population and in the academic community—not to mention the unavoidable loss of self-respect.

The bureaus will be able to regain their status as guides to moral action to the extent that they succeed in extricating themselves from special publics and clienteles. They must be able to show that they are concerned with the welfare of *all*, not with that of special client-groups. This implies a humanistic approach to social and political phenom-

ena—an understanding of the human conditions that form the basis of events.

Empirical Orientation

The older tradition of the bureaus was more empirical than that of political science departments. The bureaus carefully observed political practices and events so as to compare them to the normative models of their theory. This was therefore an empiricism of fact-finding and exposé. But the theory itself remained largely untouched by empirical findings. Ultimately, discrepancies between theory and known fact contributed greatly to the decline of bureau work. Clearly, effectiveness of empirical work depends to a very large extent upon its relevance to theory. While there is always a place for the job of fact-finding, no sustained program of research and no research institution can be for long maintained unless the data can be put into a meaningful relationship to general theory.

Whether such relationships actually exist, and how firm they are, is often difficult to decide. In the daily life of experience, the variables a researcher would like to observe are muddled and obscured by fortuitous conditions. Complex techniques of data-gathering and interpretation are often needed to make the needed data available for inspection. Existing techniques must therefore be inventoried and adapted to needs of research in our area. Currently, knowledge of formal research procedures and the ability to adapt them to special problems are still scarce among political and administrative researchers. The bureaus might well perform an intellectual brokerage function in making the new techniques available to the profession.

Interdisciplinary Approach

In the past, a common political ethic enabled the bureaus to attract not merely political scientists but also economists and lawyers. This interdisciplinary cooperation, which helped to explore more fully the phenomena of politics and government, has now largely disappeared. Even in the field of political science, the number of professionals willing to work in bureaus has now greatly shrunk. The loss of a central ethic is not the least of the reasons for this isolation of the bureaus in the academic community.

The disciplines attracted to bureau work in the past were largely the "hard" or "tough-minded" (to use William James' term) areas of the social sciences—those which did not deal explicitly with man and his hopes and fears but rather with money, commodities, legal norms, and precedent. A new humanistic ethic of governmental research would

seem to call for much closer cooperation with the "soft" or "tender-minded" disciplines, such as psychology and sociology. Besides, these fields have in recent years taken great strides and offer findings of considerable use in the understanding of political events. While some researchers have endeavored to exploit the "tender-minded" social sciences for sheer managerial manipulation, it would seem peculiarly the task of the bureaus to use their findings for a more penetrating understanding of the problems of our time and for the development of those plans of social action which are consistent with a humanistic ethic.

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Comment

TO THE INQUIRING MIND, there is nothing quite as stimulating and challenging as a frontal attack, unless indeed it be the subtleties of flanking and probing maneuvers. Dr. Pinner's paper presents the best of these devices. To be sure, less anxiety might have been aroused if the attacks had been directed toward the outer breastworks rather than toward the heart of the citadel, toward something vague and impersonal rather than toward specific beliefs long held and highly cherished. Dr. Pinner's intellectual honesty, however, need give rise to no anxiety neurosis even though his attack strikes at some of the roots of traditional political science.

At least to this writer, it seems logically correct for Dr. Pinner to conclude that: "at the present time, the prospects for systematic political theory do not appear promising" and that "political researchers will have to look to other, more highly systematized areas of the social sciences . . . for support." This truth has long been inherent in the discipline known as political science. It is clearly evident in the Great Names of political philosophy. It does indeed compound the problems involved in developing a "systematic theory."

But as I read the literature of the "more highly systematized areas of the social sciences," I find conclusions extremely difficult to reach, and my certainty regarding the truth contained in their findings is in inverse proportion to *their* certainty.¹

Dr. Pinner's discussion of anxiety and avoidance seems to me to be a case in point. Hobbesian-based, but dressed in modern language, his discussion could, it seems to me, be sharpened considerably.

Why is there "no reason to suppose that social interaction is, as such, likely to be reinforcing"? Why is it supposed that all group-productivity will result in "counter-balanced group-generated anxiety"? It seems this is the thesis-antithesis argument *reductio ad absurdum*.

Again, what does Dr. Pinner mean by: "Now political groups and—even more so—the apparatus of government are largely nonproductive"? Just because "the relationship between behavior and reward cannot be easily perceived," it does not follow there is, or can be, no relationship. Anxieties may or may not be generated by the "apparatus of government" when it collects the garbage, paves the streets, constructs and

¹ See J. J. Schwab, "What Do Scientists Do?," *Behavioral Science* 5: 1-27 (January 1960).

operates the schools, creates a public health program, maintains "law and order," and provides for "domestic tranquility."

As Dr. Pinner points out, "political processes are . . . likely to be both products and sources of anxiety." May they not also be both products and sources of great satisfactions? Perhaps the Hobbesian view of life is neither the correct nor the final view of life? It may be that the "systematic exploitation of anxieties" can only lead to the destruction of collective effort. But it may be, too, that anxieties are far less basic than is suggested by Dr. Pinner's analysis.

A deeper question seems to appear. Does it necessarily follow that "differences in personality structure" make "a balancing of group demands" an improper tentative frame of reference for political scientists? There are, so it seems to me, other and better reasons for rejecting the equilibrium postulate.² But does it necessarily follow that individual personality structures determine group structure, group action? To raise the question in this form indicates the complexity of political life.

In some ways, the dismissal of concrete historical settings is surprising. Questions may be raised without regard to such settings, it is true, but how does empirical verification take place outside the setting of a particular moment in time? Experimentation is an easy answer. And right at this point I find myself slightly schizophrenic.

Dr. Pinner's discussion on integration and segregation, followed by his sections on methodological suggestions, is most suggestive and constructive. But the schizophrenia derives from my inability to find a clear and positive answer to a basic question: Namely, how do we move from empirical data to the theoretical framework and *vice versa* without forcing our theory upon reality, a sin from which political science has suffered since the days of Plato?

As I read Dr. Pinner's paper, I think he would agree the theoretical framework must bear as close a correlation to observable data as the state of our technical competence, or the "state of our arts," will allow. Our theoretical framework will suffer in direct proportion to the bluntness of our tools. In this connection Dr. Pinner properly reminds us of the real progress made in recent years.

What really bothers me seems to lie in the implicit, rather than the explicit, posture taken by Dr. Pinner's paper. I wonder whether the right questions are being asked? Is there an underlying assumption which pre-determines our view of man, as an individual and as a participant in the collective political enterprise? Does this assumption shape the questions asked of "reality"?

² See Stanley Rothman, "Systematic Political Theory," *American Political Science Review* 54: 15-33 (March 1960).

Is the assumption that of a determinism which leaves the individual with only the "illusion" of choice and responsibility? But may not the "illusion" be a fact? The difficulties psychiatrists encounter with the id-ego-superego indicate the kind of question which haunts me. So too does the discussion of behavior trimorphism and, for the same reason, of implied determinism. It is not determinism alone which bothers, but rather the lack of an explicit referent. The self-fulfilling prophecy is everywhere present in the social sciences. Is it present in Dr. Pinner's view of man?

Truth endures in the minds of men and is transmitted from generation to generation through both understanding, and, unfortunately, misunderstanding. Whether it be the former or the latter depends not so much upon the data presented and the logic involved as upon the language used in presenting the case. I would suggest a reading of George Orwell's essay in this connection.³

Advance in knowledge and understanding comes through shared experience. Sharing implies receptivity as well as willingness to give. How can sharing come about if the language implies either a single road to salvation or an inability to communicate in the language of a different tradition? For all I know Dr. Pinner's road may be the road to salvation. Personally I think I like the road. But I cannot bring myself to believe that previous generations have contributed little to our understanding. Nor does Dr. Pinner think so. Implicitly, it seems to me, he has taken a combination of Hobbes, Hegel, and Marx as basic to his point of view. This I grant he is certainly free to do. It seems that Dr. Pinner may be saying that any theoretical framework other than the one he has selected for his purpose is not worthy of the name of theory. If he is saying this then it is respectfully submitted there are other equally imposing possibilities. If he is not saying this, then I have flailed a straw man and ask Dr. Pinner's forgiveness.

Turning to his last section on the appropriate role of a university bureau of governmental or political research, I respectfully suggest Dr. Pinner has, in some ways, forced the facts into his frame of reference. His description of the theoretical justification for such bureaus, historically, may apply to some bureaus. But for many, the historical programs were developed on a pragmatic basis. What personnel was available, interested, and competent? What "problems" were pressing for solution? What "assistance" was desired and from what levels of government? What was the "service" tradition of the parent institution? What legis-

³ See George Orwell, "Politics and the English Language," Maurice R. Stein, Arthur J. Vidich, and David Manning White, *Identity and Anxiety*, Glencoe, Ill., Free Press, 1960.

lative limits, if any, were explicitly placed on the research funds available? How did the basic teaching responsibilities of the academic departments influence the development of the bureau program? Questions of this sort did far more to mold the research, training, and consultative programs of the bureaus than did any systematic theory regarding their proper functions. Indeed, one is tempted to hypothesize that the theory developed after the fact—as a rationalization.

It still remains the imperative of a tax-supported institution of higher learning to place its specialized knowledge at the disposal of the society out of whose sustenance comes the support for the collection of scholars known as a university. The knowledge may be imperfect; it may be fragmentary; it may even be erroneous. But it is the best society has with which to attempt solutions to its pressing problems. And we cannot wait for the development of a systematic theory. Time presses in upon society and events demand solutions, however temporary and imperfect.

The demands of special publics and special clienteles, as apparently understood by Dr. Pinner are no different basically than those of foundations supporting "research." The physicist and the chemists can bear striking testimony to these special publics and their influence upon research. So too can the social sciences. And what makes Dr. Pinner so certain that the interests of the County Commissioners faced with a regional zoning problem are less than those of the welfare of *all* the people?

The "decline of bureau work" is a highly subjective phrase. What Dr. Pinner seems to be saying is that the old frame of reference is no longer applicable. Therefore, empirical verification is impossible within that frame. Perhaps, and perhaps not. At all events, his plea that the bureaus use and make available to the professions these newer techniques is well taken. I would only suggest it is being done to a far greater extent than Dr. Pinner postulates.

Furthermore, interdisciplinary approaches are, so it seems to me, far more common today than generations ago. This seems true if only as a function of the preparation of the new generation of scholars in the social sciences. Increased emphasis upon the "soft" or "tender-minded" disciplines is clearly evidenced in the products of the bureaus.

It seems reasonable, then, to look forward to a future where the bureaus will add a new dimension. They cannot escape their historical antecedents nor should they withdraw from the pleas for help. They owe society their wisdom, insights, and assistance, however tentative they may be. They also owe to society a dedication to improve, perfect, and verify their knowledge. And by compelling us to re-evaluate our programs, Dr. Pinner's remarks serve us well.

Summary Statement

by Conferees¹

University bureaus of governmental research—known by various titles and varying in their fortunes—have had in common an orientation toward service. In most instances institutional commitments and public expectations will properly require a continuation of the characteristic consultative, informational, and training functions. But bureaus exist in an academic environment and the service work they do should be of a type that is congenial with university traditions of scholarship and, if possible, contributory to the advancement of learning. A major objective of the bureaus should be to make service and research functions complementary.

GOALS

What is the purpose of a bureau of governmental research? What are the goals to be pursued? To what do bureau directors owe their allegiance: To the service of demanding clienteles? To the advancement of knowledge? To the enlightenment of the citizenry which makes possible the university as a community of scholars?

Most directors will agree that there is no clear and simple answer. But they will agree also that there is no necessary and essential antagonism among these various goals; that they need not be viewed as alternative courses of action, but to some extent can be made mutually reinforcing. There are certainly opportunities through conscious and careful control of limited resources to contribute more effectively to the advancement of systematic knowledge.

FUTURE DEVELOPMENT

Historically, three major factors contributed to the strength of the bureaus:

1. A drive to collect and marshal governmental data.
2. An orientation toward "action."
3. A widely known and generally agreed-upon body of theory for the analysis and reform of governmental institutions.

¹ While this summary statement was unanimously approved by the conferees, they are of course aware that bureaus and institutes of governmental research vary in character and function and that not all the points contained in the statement apply equally or universally.

The passing of time has affected each of these factors.

Accordingly, the conferees believe that bureaus now need to develop further the theoretical foundations of their work. This they may do by:

1. Stimulating the development of basic theory relevant to governmental phenomena.

2. Surveying theoretical formulations in the social sciences, so as to make relevant bureau work consonant therewith, and, therefore, additive to whatever extent is possible.

3. Developing techniques for recognizing—in cases in which the research motive is “diagnostic”—which theories of behavior, gleaned from the social sciences, may apply.

The conferees believe that the effectiveness of empirical work depends, to a very large extent, upon its relevance to existing theory and the modification of existing—or the construction of new—theory where existing theory is shown to be inadequate. Although fact-finding is basic, no sustained program of research and no research institution can be fully effective in contributing to knowledge unless the data can be put into a meaningful relation to theoretical concepts.

At the same time, the conferees are aware of the dangers of overconfidence in the possibilities of theories which would apply to the complexities of real-life politics and administration. Gains in the understanding of political structures and events will depend largely on the skill of bureau research personnel in selecting, from the available store of theoretical knowledge, such propositions as will be most useful in illumining the many aspects of the specific political events under investigation.

The conferees note the recent advances in systematic methods of data-gathering and processing and in measurement, which are applicable to political phenomena; they believe that the judicious use of such methods might result in a considerable increase in precision and in economies, both intellectual and financial.

The potentialities of interdisciplinary approaches to political events appear greater today than ever before, and the conferees believe that such approaches should be increasingly followed by bureaus of governmental research. In particular, possible contributions of psychology and sociology have not been as fully exploited by the bureaus as have those of economics, law, and history.

UTILIZING AVAILABLE RESOURCES

A governmental research bureau must make optimum use of the resources available to it so that more time and more talent can be brought to bear on the research from which will emerge significant

contributions to knowledge. If the bureaus utilize their assets with imagination and skill and if they take advantage of the contributions of related disciplines, they will assist in developing an exciting "atmosphere of research." In this connection, the conferees believe that:

1. The academic status of professional bureau research personnel should be of an order that can attract qualified scholars.

2. Increasing integration of departmental and bureau personnel is a goal to be sought. As a long-range objective bureau research personnel should have qualifications of such a nature as to facilitate cooperation and integration with academic departments.

3. The "rotation" of research-oriented personnel by the increased use of short-term or project commitments in bureaus is desirable to stimulate new ideas for research and new approaches to research problems.

4. Increasing interdepartmental or interdisciplinary appointments will help to bring an exciting sense of research possibilities into specialized or traditional areas of research.

5. Increased use of research seminars and meetings in the bureau setting will be stimulating, particularly if such seminars are part of a formal graduate program of studies.

To move along these lines may involve costs beyond the present resources of individual bureaus. The conferees believe the strengthening of staff resources is of such significance that it warrants foundation support until normal financing can be increased.

Changes in administrative practice alone are not sufficient. A consciously stated purpose of bureau action is needed. The conferees believe a primary function of the bureau director is to lead his colleagues in the development of a consensus as to the purpose and priority of research endeavors.

PROPOSED RESEARCH ACTIVITIES

Research activities for the bureaus are suggested in the several papers, in the comments, and in the various proposals advanced during the sessions. Although these proposals cover a wide variety of fields, there was most attention and agreement on the current and special importance of imaginative research on:

1. The nature of political behavior and processes including such matters as patterns and structures of power and influence, determinants of political innovation and change, political attitudes, and leadership.

2. Problems of public policy in substantive areas.

The conferees further agree on the metropolitan and urban complexes as a desirable and useful focal point for the study of political

behavior and of public policy. In the study of the urban problem and in other process and public policy inquiries, attention should be given to the regional, national, and in some instances, the international aspects of the subject matter.

RESEARCH FACILITIES

The research libraries in bureaus have provided important facilities for a variety of university and governmental research. These libraries need to be complemented by the collection of basic data that will provide fundamental information for long-term studies on politics and administration. Comparative studies in government will require additional facilities for exchange of study designs and data. These and other general research facilities can provide an important contribution to university and governmental research and to university instruction.

INFORMATION EXCHANGE

The conferees believe that existing communication between bureaus is inadequate and urge the exploration of possibilities of a more systematic means of exchange of information on research plans, projects, and completed studies. Such exchange might make comparative studies more feasible and more useful. Additionally, in some areas bureaus might find it profitable to plan and conduct joint research projects.

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